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SOME NOTES ON EARLY MIGRATIONS IN THE SOUTHWEST PACIFIC AREA

FELIX M. KEESING

THE RESEARCH and training programs of the war period focused the attention of some of us more intensively than before upon the islands in the Malaysia-Australia zone, and the military problems concerned called for collating materials not only from anthropology but also from geography, geology, botany, and other natural science fields. Inevitably, in analyzing conditions relevant to the movement of our forces *westward* through the area, our attention was called to many factors which might yield fuller understanding of the earlier movements of peoples *eastward*—the migrations by which the Australia-New Guinea region was originally settled.

Published studies by anthropologists to date on migration problems in the southwest Pacific have almost without exception been vague regarding geological, climatic, and other conditions at earlier time levels, at least for the region beyond Java. Most of them bypass more or less entirely the question of how early man actually made the transit across the intervening channels and sea spaces to the Australian area. Some speak vaguely of geological subsidence by which former pathways may have been drowned. Even such a careful scholar as Cole, in his *Peoples of Malaysia,* speaks in general terms of "changes in land and sea levels" with little attempt at specific explanation; "the fact is that [the peoples concerned] are there and did arrive either by sea or overland bridges."[1] A few, including the writer, have noted as an apparently relevant factor here the lowering of ocean levels during glacial periods, as having exposed land "bridges" and narrowed channels between islands to a greater extent than today. The most extensive dis-

1 Cole, 1945, p. 5.

cussion of the significance of such eustatic movements is offered in a paper by Birdsell in which he deals with the origins of the Australian Aborigines.[2]

By contrast, much closer attention has been given to the paleogeography of the region by botanists, zoölogists, entomologists, and other natural scientists interested in historical and distributional problems of plants and animals in the southwest Pacific. Such scholars as Merrill, Dickerson, Lam, Mayr, and Raven, to mention a few outstanding names, have correlated their data with the findings of geologists and hydrographers such as Brouwer, Rutten, Umbgrove, and Vening Meinesz.[3] The notes which follow suggest what fuller coöperation with all these kinds of specialists may contribute potentially to the anthropologist interested in human migration problems.

For studies of early man in the Java region, the *Pithecanthropus* types and their successors, it has been necessary to go back to lower Quaternary levels. Java served as a kind of catchment or pocket into which hominid and homo migrants funneling southeastward out of Asia have backed up against the edge of the Asiatic continental shelf.[4] For the areas eastward of here, however, it appears extremely unlikely that the dating of human occupation can be placed back of the fourth glacial.

Several scholars have suggested an earlier time level for the first migrations into the Australian region, the most notable being Mahony who studied the geological setting of the fossil Keilor man in southeast Australia and considers its chronological position as third interglacial.[5] This would appear to require the arrival of the earliest migrants during the third glacial, as will be seen. It would also push back the dating of *Homo sapiens* farther than is usually accepted, with the alternative of counting the modern Australoid-Negritoid type populations as of pre-*sapiens* ancestry. The latter viewpoint, though proposed by some of the early scholars, has little support today and is definitely opposed by recognized specialists such as Birdsell, Howells, Tindale, and Wunderly. The Keilor find, together with comparable fossils—Talgai and Aitape "men"—in the Australia-New Guinea region, and Wadjak man in Java, all fall within the neoanthropic range. For the purpose of this analysis, therefore, the fourth glacial will be con-

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2 Birdsell, 1949b.

3 Useful diagrams relating to paleogeographic data are found in Honig and Verdoorn (editors), 1945, pp. 25, 34, 37, 243, 322; see also *Atlas van Tropisch Nederland*, 1938.

4 Von Koenigswald, 1937; Movius, 1944, 1949; Weidenreich, 1946, 1950, and other works.

5 Mahony, 1943a, 1943b; see also Zeuner, 1944. Sir Arthur Keith (1931) also suggests very early migrations. Weidenreich disputes vigorously the geological interpretation which suggests dating the Keilor find back of the last glacial (1950, pp. 232-235).

sidered the reasonable baseline. Should fuller knowledge on the early horizons of *Homo sapiens* make it appear feasible that some groups reached the southeast Asia pocket by the previous glacial the reasoning in this paper could easily be extended to cover the case.

Six major physical factors have to be taken into account here: (1) the general structure of the Malaysia-Australia zone, (2) possible tectonic shifts, making for both subsidence and uplift, (3) the dynamics of local vulcanism, (4) eustatic

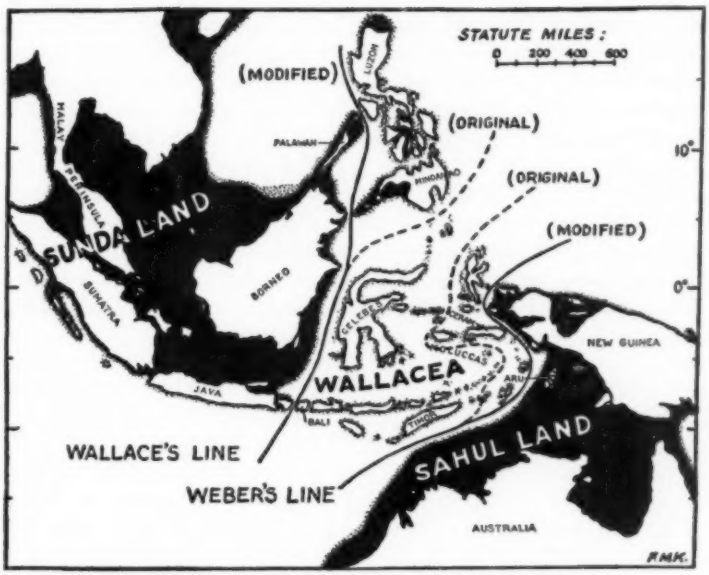

FIG. 1. Pleistocene land-masses in the Southwest Pacific.

movements affecting the distribution of land and sea spaces, (5) the effects of currents upon inter-island channels, (6) climatic changes, with correlated modifications in flora and fauna. These may be considered briefly and selectively in relation to the migration problem.

The Malaysia-Australia zone, extending from the Eurasian land mass much like a vast partially submerged peninsula, is best understood structurally as comprising three major regions (see Fig. 1):

(a) The Asiatic continental shelf. This, often called "Sunda land" in the

literature, has along its heavily folded edges the Great Sunda islands and their outliers: Sumatra, Java and Bali, Borneo, and Palawan Island in the western Philippines. Much of the ocean floor here is shallow, and if the present sea level were lowered even by about 150 feet it would be possible to walk dryshod from the Asian mainland to all of the islands mentioned.

(b) The Australian continental shelf, correspondingly called "Sahul land," or sometimes "Papua land." This great platform, also shallow, not only links Australia with New Guinea, and with their western outliers such as the Aru Islands, but also reaches fairly close to Timor Island. It would need a recession of sea level of merely some 65 feet to provide land bridges between Australia and New Guinea. Away to the southeast of Australia it would correspondingly require a drop of about 180 feet to connect Australia with Tasmania (Bass Strait).

(c) The intermediate area between these shelfs. This is sometimes called "Wallacea," a name coined by the botanist Dickerson and associates in memory of the pioneer British naturalist Wallace. This is a highly folded area, pitted by great sunken deeps sometimes up to 16,000 feet, cut by channels some of which are of long geological antiquity, and with complex arcs and clusters of islands many of which rise to great heights. It comprises most of the Philippines, also the Celebes, Moluccas, and Lesser Sundas west of Bali. This zone is naturally of crucial interest to the migration problem.[6]

During the period since early man would have reached the Pacific littoral, the main shield areas of the Sunda and Sahul shelfs have been orogenically stable. Their outer edges, however, have been subject to marked tectonic shifts along with other rim areas of the Pacific deep. Fault lines still marked by volcanic activity run along the southern edge of the Sunda shelf (Sumatra to Bali) and the northern edge of the Sahul shelf (north-central New Guinea to New Hebrides). These dynamic factors are greatly magnified in the Wallacea area. This zone, all geologists recognize, has been exceedingly unstable through the Quaternary, with processes both of subsidence and of uplift still under way, and with

6 Geological studies of the region are best summarized in Brouwer, 1925; Umbgrove, 1938; Stauffer, 1945. The area to which the term "Wallacea" is applied here extends somewhat eastward of the botanically significant region as originally defined by Dickerson (1928). It corresponds to that delineated by the zoölogist Mayr (1944), and is about equivalent to what Rensch (1936) called the "region of intermediacy." Critical to such discussions are the placing of the so-called Wallace and Weber lines and their subsequent modifications by later scholars such as Huxley, Dickerson, Merrill, and Mayr: see Mayr, 1944; Merrill, 1945. The geology of the Australian-New Guinea region is summarized in Mahony, 1943a; this has an extensive bibliography.

marked vulcanism through the inner Banda arc (western Lesser Sundas), and from the western Moluccas through the northern Celebes to parts of the Philippines.[7] Repeated periods of violent tectonic activity have occurred, alternating with ones of relative quiescence. These appear to have provided intermittent land connections between islands, though at no time a complete bridge between the Sunda and Sahul shelfs. Studies by paleobotanists and paleozoölogists are particularly relevant here.[8]

The geodesist Vening Meinesz and others have suggested that the general tendency in the Wallacea area has been toward subsidence. But all such specialists also point out what the late Dr Bailey Willis, eminent Stanford geologist, emphasized particularly in a number of conversations with the writer concerning the region, namely the need for taking full account of the local character of orogenic movements: some block structures sinking while others were rising, coastlines warping differentially, and so on.[9] These localized dynamics, together with the factor of local vulcanism, would become of extreme importance for any investigator trying to pinpoint likely places to search for sites of early human occupation—more of this later.

Eustatic movements offer what appear to be an even more important general factor in building up the picture of early human migrations. During a glacial period the freezing of a greater amount of the earth's water around the poles and on heights, accompanied by related isostatic shifts resulting from the differential weights of land and water zones, brought about a marked recession of the sea level in the region, while interglacials brought it well above the present height.[10]

The importance of such eustatic movements up to the Late Pleistocene has been taken into account in studies of the early hominid populations found in the upland Java valleys, and accompanying paleontological and other data, as in the works of De Terra, von Koenigswald, and Weidenreich. For the movements of *sapiens* groups, as already indicated, probably the fourth glacial only is significant; the third glacial appears too early, and the water spaces of an interglacial would be an even more effective barrier for peoples at the hunting and gathering level of the time than those of today. Geologists and hydrographers have estimated for this fourth glacial a recession in the area of between 150 and 300 feet below

7 Ter Braake, 1945; this gives several maps relating to vulcanism.

8 See Bibliography.

9 Cf. Willis, 1937; Umbgrove, 1938; Stauffer, 1945. See also the zoölogist Mayr, 1944; the botanist Merrill, 1945. In the Timor region, Quaternary uplifts are vividly demonstrated by marine terraces elevated in places to over 4,000 feet.

10 General discussions of the principles involved are found in Daly, 1934; Flint, 1947.

the present sea level; Daly accepts a figure of 75 meters, or about 240 feet.[11] Even apart from orogenic changes (above), this would have permitted any wholly land-migrating groups who were in the Southeast Asia region at the time (i. e., from perhaps 30,000 to 20,000 years ago) to reach dryshod all the important islands on the Sunda shelf. For the zones further east, however, the situation is necessarily more speculative. While it seems reasonably certain that the original Australoid-Negritoid type populations made their way through Wallacea to the Sahul shelf during this glacial, the specific distribution of land and water in the area at the time is still not well understood, hence the actual pathways and means of transit are matters of surmise only.

A striking feature of this intermediate zone is the multiplicity of possible paths for human migration, subject to the postulates of lowered ocean levels and possibly uplifted land passages. They weave not only from Java along the Lesser Sunda chain to Timor and other islands adjacent to the Sahul shelf, and so to Australia proper, but also northward from them to the Celebes and Moluccas, and also from Borneo to the Philippines and so to the Celebes and Moluccas, then on to the Arus and New Guinea. The evidence from paleobotany and paleozoölogy seems clearly to indicate that at no time did a complete land passage exist continuously through the area, but rather that sporadic interconnections occurred in different island zones at different times.[12] Whatever use early human migrants may have made of such temporary land bridges to advance their eastward progress, it appears certain that they would have had to cross some water channels, notably at the so-called Wallace and Weber lines.[13]

A further imponderable here is the dynamic effect of sea currents in scouring and filling waterways in the area. This has so far received surprisingly little reference in the literature. The dominating feature in the contemporary picture is for the great South and North Equatorial Currents, crossing westward from the American side of the Pacific, and backing up along the Australia-Melanesia-Wallacea shores, to pour much of their waters relatively swiftly (though with seasonal variations depending on wind directions) through the Wallacea area. Entering through the straits systems between Australia-New Guinea, New Guinea-Moluccas, Moluccas-Philippines, and southern Philippines into the complex basins and channels of Malaysia, such waters pass partly northward into the China Sea,

11 Daly, 1934, p. 182. See also Umbgrove, 1929; Kuenen, 1935; Raven, 1944; Merrill, 1945; Taylor, 1946.

12 Cf. Merrill, 1926, 1945; Dickerson, 1928; Raven, 1935; Lam, 1938; Mayr, 1944. The last named reference has a useful discussion of the main straits in the region looked at from the viewpoint of paleozoölogy.

13 See footnote 6.

and partly southwestward through the series of straits leading into the Indian Ocean.[14] During glacial times, however, the situation would have been markedly different: the Torres Straits and those on the Sunda shelf closed; some of the shallower Wallacea channels closed, silted up, or at least narrowed significantly; perhaps others, even if narrow, more deep and swift than now. In turn, with decline of the glacial, the gradual flooding in of waters would tend to scour away former land bridges and make some of the continuing channels wider and deeper. Such speculation emphasizes the importance of localized hydrographic studies for understanding of the migration problem.[15]

The assumption that some water crossings were necessary for human movement outward from the Sunda shelf provides in turn a challenge to ethnology. The Negrito peoples in the major islands of the Philippines, which are at present separated from the Sunda shelf by narrow but deep channels, are *par excellence* land-dwellers, though they are often found living along streams and in swampy areas. But some of the modern Australian Aborigines used crude canoes or rafts of logs, bark, or rushes. The Tasmanians also had primitive canoes seaworthy enough for them to reach nearby offshore islands.[16] This suggests that channel crossing may have been done by elementary watercraft such as these; the alternative is to visualize the use of logs for buoyancy, or else swimming.

For most crossings the distance would be a few miles only. Furthermore, high islands would loom clearly ahead so that no great venturesomeness would be called for. One notable exception to this, however, would be movement directly from the Timor Island area to the adjacent flats of the Sahul shelf, where no land would be in sight. The exceptional distances here, too—apparently nowhere less than a hundred miles even at the glacial maximum—make this pathway more dubious than the apparently narrower ones further north in the Tanimbar-Kei-Ceram-Aru-Vogelkop area. Yet even use of such northern routes may well have

14 The most authentic sources here are the United States Hydrographic Office Sailing Directions and related charts on the region.

15 The writer, while claiming no expertness relating to the parallel problem of the migrations of man to the American continent, is struck by the apparent lack of attention in the literature to localized hydrographic conditions in the Bering Strait area. Since it is recognized that the shallow strait would have been an exposed land bridge, what about the effects on the current and channel situation farther south in the Aleutian area? How far southward would the ice bridge have consolidated, without the present Bering Strait flow, and what would be the physical conditions in such an ice zone; hence, how far south could an ice crossing have been feasible, perhaps using the Aleutian chain, from Kamchatka, directly to the Northwest Coast? What local uplifts might suggest likely points for archaeological study along raised shore faces? In what areas would it appear hopeless, in view of eustatic changes and subsidences, to carry on such investigations? (See also footnote 19.)

16 Davidson, 1935; Elkin, 1943, pp. 5-6; Mahony, 1943a, p. 12.

needed the ability to cross one or more channels up to perhaps sixty miles wide on the way from the Sunda shelf,[17] so that the use of some kind of temporary watercraft appears to have been necessary.

The factors of climate and of flora and fauna are worth adding to the speculative elements here, as they may have served as important impulses in these migrations, particularly as regards food supply. The Malaysia-Australia area is one of the classic monsoon zones. It has today a complex pattern of climate, with related plant and animal populations, varying particularly according to how sides of islands are affected by the northeast monsoon, which is dominantly wet as traversing sea areas, and the southeast monsoon, which is dominantly dry, especially in the southeastern islands; here the sides nearest the great dry land-mass of Australia are mostly parched grasslands.[18]

At a glacial maximum, with the Australia-New Guinea continental platform exposed to a larger degree than now, the Sunda shelf also largely exposed, and the water spaces in and around the Wallacea area adding moisture to the winds lessened in extent, the climate would indubitably have been drier. Especially during the southeast monsoon season, the dry and relatively barren conditions found on the sides of the eastern Lesser Sundas facing Australia would have been characteristic also of areas considerably further north and west. The northwest monsoon, however, would presumably be drier only in the island areas on or near the Sunda shelf, and perhaps in southern Wallacea; the northern and northwestern sides of the other islands would receive about the same rainfall as now because this monsoon would be curving over approximately the same water spaces northward of Wallacea as now.

What would this mean in terms of flora and fauna, hence of food supply and other conditions favorable or otherwise to migration? The climatic shifts just pictured would mean, for one thing, a widened distribution of grasslands and of the monsoon type forest, with its limited range of bamboo, palms, and other vegetation capable of surviving alternative dry and wet seasons.[19] This type of habitat is comparable with some in which present-day Australian Aborigines sur-

17 This figure of sixty miles is suggested by the geographer Taylor, 1937, p. 73.

18 United States Hydrographic Office, *op. cit.; Atlas van Tropisch Nederland,* 1938; Braak, 1929; Setchell, 1929; Mohr, 1938.

19 *Handbook of the Netherlands East Indies,* 1930; van de Koppel, 1938; Merrill, 1945. As an addendum to footnote 15, dealing with migrations to America, data from climatology and paleobotany would also be relevant: the closing of the Bering Strait and establishment of extensive ice sheets in the present Bering Sea would have modified winds and other climatic phenomena. Here food supply studies would have to take account of the accessibility of marine and other fauna, especially if a southern ice pathway were being postulated.

vive as hunters and gatherers, though its resources of plant foods and game are scanty, unless marked dependence upon marine foods is included, as would probably have been the case within early migrant groups. Correspondingly, the zones of tropical rainforest would be lessened in extent, especially in the extremes of east and west Wallacea, and on the southern sides of islands. It seems fair to speculate that the northern faces of islands would, in general, be more habitable than the southern; the latter do not even get the benefit of the rainy northwest monsoon in the case of the higher islands.

In weighing the implications of such data, the hypothesis suggests itself that these factors may have given impetus to human migrations through the region. Once early man ventured out into the Sunda shelf and Wallacea areas, the pressures and limitations of food supply along the narrower edges and slopes of these generally mountainous islands may have impelled some groups onward, even to hazard the crossing of intervening water channels. Here the anthropologist needs to work with the paleobotanist and paleozoölogist to weigh the factors involved, again taking specific account of localized conditions.

What then, in sum, are the implications of these materials from the natural science fields? First, they give a realistic physical context within which human movements in the region can be postulated without undue vagueness or resort to migration myth and magic. Second, they pose significant problems for further research.

First, it has been suggested that the early migrations through the Wallacea zone can best be accounted for by the general recession of ocean level in glacial times, though with localized orogenic movements undoubtedly playing a role in facilitating them. Once land pathways became flooded and channels widened, such hunting and gathering populations would seemingly have been unable to traverse this region. Further migration would then have had to await much fuller development of the arts of oceanic navigation, as characteristic of later Indonesian (or as some prefer, Proto-Malay) peoples and Neolithic times.

Again, the data suggest a multiplicity of possible routes of travel, both in leaving the Sunda shelf and in passing eastward to reach the Sahul shelf. Some early groups may have debouched at points most favorable to a southward spread into Australia, others at more northerly points which would favor settlement in the New Guinea area. The likelihood is that the deepest and widest channels were crossed at times approximating to the height of the glacial. Furthermore, because Tasmania, away to the southeast of the Australian continent, was apparently occupied before being cut off unduly by rising waters and scouring currents, it would seem that early migrants had penetrated through to that marginal area by

at least soon after the glacial maximum.[20] The final dispositions of population, however, as between the Aru Islands, the New Guinea area, and Australia, could have been delayed until the glacial decline was well along and the shallow Torres Straits had opened up.

What might an anthropological investigator, or better a team which could bring together expertness from the major scientific fields concerned, look for in tracing signs of the passage of early man in the Wallacea area? First, certain limiting factors would have to be recognized. If the movements occurred during a glacial period, the actual pathways of migration are sure to be in part now below the present sea level. Where subsidences have taken place, too, such as warping down of coastlines, these would tend similarly to drown such pathways, while in zones of local vulcanism they may have been irrecoverably obliterated by subsequent deposits. Such factors, however, could have some positive usefulness in helping to delimit promising sites for research in so vast an area.

On the positive side, geological investigation could suggest localities where subsequent elevation has occurred, so that travel routes would still tend to be exposed. Even in apparently drowned areas careful topographic and hydrographic studies could show places where moving groups would probably have found it necessary to climb over cliffs or ridges, or suggest gentle slopes or valleys up which they might have foraged to higher levels, comparable with such places as Trinil and Solo in Java. Again, geologic, climatic, and botanical data could suggest whether the northern or southern sides of islands might have appealed more; in the western Lesser Sundas, for example, the generally steep and barren southern sides would be harder to traverse and have less food and water. Within favorable localities, archaeological reconnaissance might in due course reveal evidence of campsites and artifacts, especially in rock shelters and caves, and these in turn point the way to associated skeletal finds.

A special challenge in this regard comes from the meagerness of pre-Neolithic archaeological data, and indeed of systematic archaeological investigation, within the Wallacea region. Heine-Geldern has recently summarized effectively the sporadic work done by the Sarasin brothers, van Stein Callenfels, and other prehistorians in this zone of Indonesia.[21] Apart from Neolithic materials, the few finds

20 This indicates acceptance here of the views of Birdsell, Mahony, Tindale and most others that Tasmania was settled via the Australian continent, and skepticism for a hypothesis given some credence by Huxley, Wood-Jones, and a few others that the Tasmanians migrated by water from New Caledonia far to the northeast. It should be noted here that Mahony is not sure that the Bass Strait was completely bridged at the time of the fourth glacial (1943a, p. 16).

21 Heine-Geldern, 1945; this has a full bibliography.

have been almost exclusively flake industries, usually microlithic, and judged to be late Palaeolithic to early Neolithic; they have come from the southwest Celebes (proto-Toalian), Roti, Timor, and several other localities. For the Philippines, Beyer has recorded on Luzon and other islands adjacent to the Sunda shelf numerous flake tools, also usually microlithic, which he associates with early populations arriving during the last glacial, but also a few roughly worked pebble tools which he equates with the Java hominid levels (mid-Pleistocene).[22] Both the writers just cited correlate these flake industries with comparable finds in Java, Sumatra, and the adjacent mainland countries. For the Australia-New Guinea area, authentically early archaeological finds are virtually nonexistent in spite of a long history of claims to antiquity for particular sites and artifacts. At most, the stone industries of later periods offer certain limited correlations, especially in the case of flake tools. The cruder Australian core tools, which some archaeologists have suggested are typologically comparable with the so-called early Hoabinhian of Southeast Asia, have no parallels in Wallacea sites to date except in Beyer's palaeolith series.[23]

Any attempt to reconstruct the number and size of migrations to the Sahul shelf must be purely speculative. The racial characteristics of the modern populations have led to the generally accepted hypothesis that at least two migrations of early man reached the Australia-New Guinea area, one the short to pygmy Negritoid and the other the heavily featured Australoid.[24] The picture here, however, is complicated by the fact that peoples of dominantly Negritoid type are found in the extreme north (New Guinea) and south (Tasmania), with those dominantly Australoid generally between.

Birdsell, perhaps the most thorough student of Australian-Tasmanian physical types, has recently postulated instead a "tri-hybrid" genetic heritage for these populations: Tasmanoid (i. e., Negritoid); Murrayian (which he considers a primitive Caucasoid type related to the Ainoid of east Asia, and linked with the Wadjak, Keilor, and related fossil finds); and Carpenterian (which he relates to the Veddoid type in south Asia).[25] He visualizes these three elements as migrat-

22 Beyer, 1948a, 1948b, 1949; see also his scattered writings from about 1926 when effective archaeological work in the Philippines was started.

23 McCarthy, 1940; Casey, 1940; Elkin, 1943, pp. 7-8; Mahony, 1943a, pp. 31-45 (this discusses comprehensively the supposed early finds in Australia, and has a full bibliography); Heine-Geldern, 1945; Beyer, 1948a, pp. 8-17; Movius, 1949.

24 Cf. Bijlmer, 1929b; Davidson, 1937a, 1947; Kennedy, 1943; Cole, 1945; Howells, 1945; Hooton, 1948. A number of other references on racial backgrounds and characteristics are given in the Bibliography.

25 Birdsell, 1947, 1949a, 1949b.

ing more or less separately to the Sahul shelf during the early, middle, and late stages of the last glacial, arriving in the order shown. Where the Tasmanoid comes to predominate north and south, the Caucasoid Murrayian concentrates in the more temperate southern zones of Australia proper and the Carpenterian in its more tropical northern sections. In his most recent paper Birdsell also suggests that the mingling of these three elements has produced what may be counted a fourth category, developed in Australia.

Racial hypotheses of the kinds here cited, though widely held, have suffered (in the writer's opinion) from inadequate spelling out of the genetic perspectives and possibilities involved. If the primary migrations took place during the fourth glacial, something like a thousand or more generations of subsequent breeding have occurred among these populations since they left the Sunda shelf. To read directly from the frequency distributions of physical traits among them today to alleged original races or types existing as separate entities at such a time, though it is almost universally done in such reconstructions even by physical anthropologists, is to let the older classificatory statics prevail over the dynamic viewpoints opened out by modern genetics.[26] Rather, account must be taken of the hundreds of intervening generations of localized inbreeding and possible outbreeding among the small groups involved, the play of complex selective forces, possibly even the appearance and stabilization of new or duplicating mutant characters. Even if hardly probable, it is theoretically feasible for the individuals in one small migrant group to have carried out of the complex racial "pool" of the southeast Asia region most or possibly all of the range of genetic characters supposedly represented in these early population elements; a few months of travel could have seen them onto the Sahul shelf, where they could then have gradually scattered out to establish local lines of breeding which, in the course of genetic "drift," would emphasize or isolate different characters and types.

With such strictures in mind, the writer prefers to reserve doubts regarding all over-positive and simple statements on the racial aspects of early migrations. The likelihood, however, is that a number of small migratory bands found their way into the various parts of Wallacea over the long span involved in the glacial period, and that some reached the Sahul shelf at different times. It is also likely, as a result of early types of differentiating *Homo sapiens* being funneled together in the Sunda land region, that most or all such bands would already show a considerably diversified range of genetic characters (or be "mixed" if we want to use this rather specious term), rather than being wholly "Negritoid" or "Austra-

26 See, for example, Stern, 1949, especially pp. 542-604; also standard works by Dobzhansky, Simpson, and others.

loid" or "Murrayian." Certainly if "mixture" could take place subsequently in the Australian area, it did not have to await the arrival of separate racial migrations. A notable point here is that Wunderly and Adam, the specialists who reported upon the anatomy of the fossil Keilor man, believe that (to quote the first-named), "it combines Australoid and Tasmanoid in about equal proportions."[27] This point, however, could in turn be pressed too far: the probability would be that some migrant groups carried a much higher statistical incidence of Negritoid type characters as they are recognized today, others of Australoid (or Murrayian and Carpenterian) type characters.

This genetic reorientation of racial history in such a region may leave the student with a feeling of greater ignorance, for want of controlling information regarding the actual breeding processes involved; but at least it rescues him from superficial or even false assumptions. For example, it largely takes the point out of a long-standing split in scientific opinion as to which racial element reached the Australian region first. On the one hand, such authorities as Birdsell, Buck, Haddon, Hooton, Mahony, and Wunderly have judged that the Negritoid came earliest, while Beyer, Davidson, Howells, Howitt, and Kennedy have been among those suggesting the Australoid. It is even theoretically possible that the highly inbred pygmy type populations found today in a few parts of the Malaysia-Australia area represent in some degree later specialized products of selection in poor forest habitats and with cultural factors favoring bodily smallness; but the widespread distribution of such populations could equally be made the basis for the more conventional view that their characteristics became segregated out very early in human differentiation.

On the positive side, such genetic perspectives also make more realistic the analysis of the later racial history of the area. Starting perhaps 5,000 to 6,000 years ago, according to Beyer's Philippine dating, canoe-borne immigrants in turn reached the Great Sunda islands and Wallacea, and in time spread eastward, especially along the northern coastal areas of New Guinea and into the Oceanic islands. These so-called "Indonesian" and "Malayan" (or, if preferred, "Proto-Malay" and "Deutero-Malay") type peoples represented exceedingly complicated breed lines, leading back in an unknown but undoubtedly significant degree to the early strains discussed above, as well as to later racial elements developed in south and east Asia. Even north Australia experienced such contacts, as witness the regular visits of Malay traders reported by the first European voyagers along that coast. Any adequate interpretation of such modern types as the "Papuan"

27 Wunderly, 1943, p. 59.

and "Melanesian," the "Alfur" of eastern Wallacea areas, Beyer's "pygmy Mongoloid," or the "Micronesian" and "Polynesian" of Oceania would have to be approached within the dynamic framework of genetic postulates.

Summarizing the general themes of this paper, it presents a wider frame of reference than is usually taken in anthropological discussions of early migrations in the southwest Pacific. The potentialities of greater inter-disciplinary coöperation in future research on migration routes are suggested; related ethnological and archaeological data are summarized; and the need is stressed for taking full account of genetic perspectives in interpreting the racial history of such a region.

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EASTERN-CENTRAL ARIZONA ARCHAEOLOGY IN RELATION TO THE WESTERN PUEBLOS

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THE PUEBLOS of eastern and central Arizona have generally been considered as local "peripheral" developments of the San Juan Anasazi. Such a view is not consistent with the known distributions in space and time of diagnostic traits of Pueblo culture. The Anasazi culture which centered in the San Juan drainage, 300-1300 AD, is a highly distinctive complex and not coterminous or identical with "Pueblo." The present paper is intended to bring out this point by a review of the distributions of diagnostic traits.

The region here included under the term "eastern and central Arizona" includes: the middle Verde valley; the area south and east of the San Francisco Peaks, from Flagstaff to Wupatki, to Winslow, and to Chaves Pass; the Salt River-White Mountains region, from the Little Colorado across the upper Salt drainage (White and Black Rivers) to the Gila. Broadly speaking, the region in question extends south from the Little Colorado to the Gila and as far west as the Verde River valley. It is primarily the forested mountain zone, along the Mogollon Rim and across the entire Salt River drainage.

In recent general reviews and popular presentations of Southwestern prehistory, this whole area has been largely or even entirely neglected. The first thing to realize is that eastern and central Arizona was about as thickly populated as the San Juan region, probably from 300 AD or before until 1400 or after.

Like the desert domain of the Hohokam in the Gila basin, the forested uplands were neglected archaeologically during the period of emphasis on the high plateau of the San Juan in the first three decades of the present century. Previously, the importance of this region had been recognized by such men as Fewkes and Hough, who did considerable work south of the Little Colorado.

In the last twenty years, however, much work has been accomplished and published in the Flagstaff-Wupatki area. The pueblo of Tuzigoot on the middle Verde, and Kinishba on White River, and the Canyon Creek ruin farther south, have been excavated and reported on. For several years, a very important program has been carried forward by the University of Arizona in the upper Salt drainage: excavations in the Forestdale valley (White River region), not far northwest of Kinishba, especially the excavation of a pit-house village of the seventh and eighth centuries, and a site of the fourth century; followed by a continuing project

in a major archaeological center at Point of Pines (Circle Prairie) in the Black River region.

In addition, studies of pottery-types, based largely on surface sherd collections, and other information from archaeological surveys, with outlining of successive horizons and of local lines of development, have been published for the Petrified

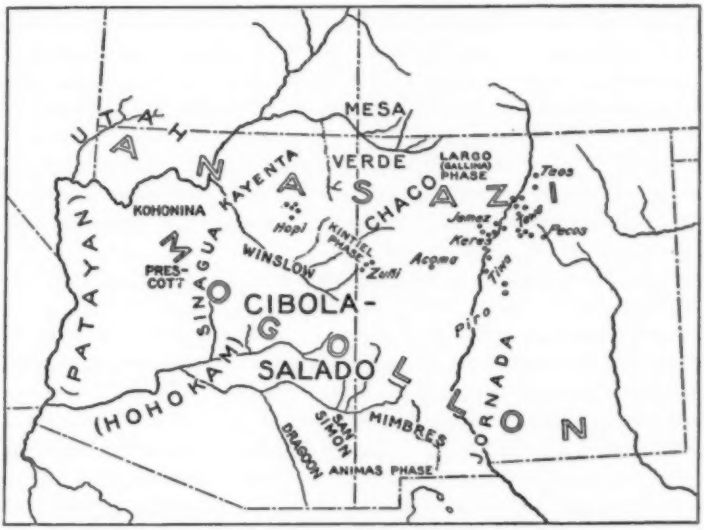

FIG. 1. Archaeological branches and historic Pueblos in the Southwest.

Forest in some detail, for the Verde-Flagstaff-Winslow region fairly fully, for the entire area in sketchy outline.

Currently, a very significant project in very early horizons of the Petrified Forest is sponsored by the Museum of Northern Arizona; and the area of west-central New Mexico, hitherto a major blank space on the archaeological map of the Southwest, is being studied by the Peabody Museum (Harvard).

Meanwhile the Hohokam culture was recognized in southern Arizona, and its sequence delineated at Snaketown; the Mimbres branch of the Mogollon culture was determined in southwestern New Mexico, and early Mogollon manifestations in southeastern Arizona were studied. Early Hohokam occupation was recognized in the Verde valley. Mogollon influence is seen in early horizons throughout the area, especially in the Forestdale pit-house site and in the Flagstaff area. Long

previously, Fewkes had noted certain distinctions between the material north and south of the Little Colorado.[1]

Nevertheless, the area has often been considered primarily Anasazi and "peripheral" to the San Juan. Conversely, the protagonists of Mogollon as a very early fundamental "basic culture" have claimed that the underlying Mogollon basis, especially in the upper Salt and even in the Mimbres, was superseded and submerged by Anasazi culture around 1000 AD or even earlier.[2] Only in connection with the Sinagua complex of the Verde and the Flagstaff area has continuity into pueblo-building times from earlier Mogollon been posited; and this western area has generally been neglected in arguments for the Mogollon complex as an early basic culture.

The general view, then, has been heretofore that late developments in the entire eastern-central Arizona area and southwestern New Mexico are primarily Anasazi, representing a local or "peripheral" form of the true Anasazi culture of the San Juan and adjacent districts.

What are the distinctive characteristics of the true Anasazi culture of the San Juan region?

Masonry pueblos constitute one such feature. This trait is, admittedly, characteristic also of eastern and central Arizona, and very probably is earlier in Anasazi than south of the San Juan.

Circular kivas are even more diagnostic,[3] missing at only one known pueblo—Betatakin—in the entire San Juan Anasazi domain. As pointed out many years ago,[4] circular kivas are lacking south of the Little Colorado valley. The sole known exceptions to date are a great kiva and an early kiva-like structure in the Forestdale valley.

Rectangular ceremonial rooms, generally lacking many circular-kiva features, seem to be present, however, in the Mimbres from perhaps before the ninth century to the end in the thirteenth century; in the Tularosa area of the upper Gila in the twelfth and thirteenth centuries; in the upper Salt in the fourteenth century and probably earlier; in west-central New Mexico quite early.

Gray corrugated utility ware and black-on-white painted pottery are characteristic of the Anasazi complex. Black-on-white is common south of the Little Colorado, but gray (reduced-fired) corrugated is virtually absent. Polished brown-

1 Fewkes, *Archaeological Expedition*, pp. 574-576.

2 Haury, *Excavations in the Forestdale Valley;* McGregor, *Southwestern Archaeology;* Rinaldo, *Conjectures;* Martin, *The SU Site;* Haury and Sayles, *An Early Pit House Village;* Lehmer, *The Jornada Branch.*

3 Cf. Kidder, *Prehistoric Cultures.*

4 Fewkes, *Archaeological Expedition, loc. cit.*

ware of various sorts, oxidized in firing, is consistently dominant, especially in the utility ware, from southeastern New Mexico across the Mimbres, the upper Gila, southeastern Arizona, the upper Salt, the Tonto basin, the Flagstaff area, and the middle Verde.

In the last two districts, the locale of the Sinagua complex, plain polished brown or red is thought to be possibly the only major kind of pottery made locally. In the Tonto basin and the upper Salt, it is accompanied by black-on-white, black-on-red, and polychrome redware, with the polychrome types superseding the black-on-white in the final period of the fourteenth century.

In the Mimbres, plain and textured brown and plain polished red pottery are accompanied in classic times by a fine "black-on-white" ware, oxidized in firing and derived from a red-on-brown type through a red-on-white phase. In the upper Gila and in southeastern New Mexico, brown utility ware is accompanied by black-on-white types reduced in firing (Tularosa, Socorro, Casa Colorado, and Chupadero).

Conversely, a decorated orangeware sequence in the plateau, indigenous in the western San Juan, accompanies gray utility pottery and carbon-paint black-on-white, with no polished brown utility. Abajo Red-on-Orange and La Plata Black-on-Orange occur with otherwise typical early Anasazi cultural remains, including Lino Gray as the relatively abundant plain utility pottery. A type undoubtedly derived from this "Pueblo I Black-on-Red" material, Tusayan Black-on-Red, is found in Northern Arizona "Pueblo II"; and Tusayan Polychrome and its subtypes in Kayenta "Pueblo III."

In these phases the utility ware is reduced-fired gray corrugated, and black-on-white pottery is fully as common as the orangeware except in the early Abajo horizon, as known at Alkali Ridge in southeastern Utah. Kivas are generally circular, axes are full-grooved, inhumations are flexed. The culture is in general as typically Anasazi as is the Chaco-Mesa Verde sequence to the east in the upper San Juan. The polished oxidized pottery of the Kayenta branch or western Anasazi in northern Arizona, "San Juan Orangeware," is thought by some to have been inspired originally by Mogollon influence from the south,[5] but this early Abajo-La Plata material is certainly indigenous, and probably authochthonous, in the plateau.

The crux of the matter ceramically is that, while rough or corrugated gray utility pottery is a distinctive feature of San Juan Anasazi, polished plain brownware is characteristic south of the Little Colorado.

5 Haury, *Excavations in the Forestdale Valley*, p. 126; McGregor, *Southwestern Archaeology*, pp. 228-230, 317; cf. Brew, *Archaeology of Alkali Ridge*, pp. 292-293.

The lambdoid variety of cranial deformation is another distinctive feature of the Anasazi culture, especially the Chaco-Mesa Verde area in the upper San Juan, occurring also as far south as Allantown on the Puerco River in eastern Arizona, and as far west as Flagstaff and southern Nevada, and, to the east, in the Gallina and the upper Rio Grande. Except at Flagstaff, where it is not dominant, it is associated with circular kivas, gray corrugated utility pottery and black-on-white painted ware, and full-grooved axes; and in the Flagstaff area there is an astonishing correlation with the next diagnostic trait to be mentioned. Vertical occipital cranial deformation characterizes southern New Mexico and eastern and central Arizona.[6]

Flexed inhumation is characteristic of the Anasazi, and also of early Mogollon sites (southwestern New Mexico and Forestdale) and the Mimbres and Jornada branches. Numerous extended inhumations occur in the upper San Juan, especially the Mesa Verde horizon; cremations, undoubtedly from Hohokam influence, occur in the Mimbres. Flexed inhumation is definitely predominant in both areas, however, and is overwhelmingly predominant, so far as known, in northeastern Arizona and the upper Rio Grande.

Extended inhumation, however, is the rite practised throughout eastern-central Arizona after the seventh-eighth century period of the Forestdale horizon. Extended inhumation is common in the upper Gila, overwhelmingly predominant in the upper Salt, apparently universal in the Tonto basin and the middle Verde, and predominant in the vicinity of Flagstaff. Interestingly enough, correlation is noted in burials of the Flagstaff area between lambdoid deformation and flexure, occipital (vertical) flattening and supination.

The finely made ¾-grooved or Gila-type axe is generally thought of as a Hohokam diagnostic. Anasazi stone axes are full-grooved or crudely-noted river-pebbles, and no true ¾-grooved axe had ever been authentically reported from anywhere in the San Juan drainage until last year.[7]

The ¾-grooved axe, however, not the full-grooved, is characteristic in southern New Mexico, including the Mimbres, and throughout the pueblos of eastern and central Arizona—the upper Salt drainage, the Tonto basin and the Verde

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6 More fully presented in Reed, *The Significance of Skull Deformation*. For lambdoid cranial deformation in Mexico, see Stewart, *The Cultural Significance of Lambdoid Deformity* and *Distribution of the Type of Cranial Deformity*.

7 Cf. Morris, *Archaeological Studies*, p. 37; one specimen, a fine Gila-type polished axe, ¾-grooved for a j-shaft, said to have been found at Chetro Ketl, is unfortunately not well documented. A small ¾-grooved axe was found in 1949 by Charlie R. Steen, Jr., at Tse-taa in Canyon de Chelly, associated with Little Colorado black-on-white pottery.

valley, the Flagstaff area, and the Little Colorado valley—as well as in the Hohokam area and northwestern Mexico.

That metates in the forested uplands of the Mogollon Rim country continued in the troughed type (as against the slab metates in bins of Pueblo III and IV in the Anasazi) has been indicated in one of the first instances (fifteen years ago) of distinction between the mountain zone of eastern-central Arizona and the plateau to the north occupied by the Anasazi.[8]

Other stone material does not seem to show consistent differentiation, so far as it has been analyzed. Bonework is generally undistinctive; the notched awl of early Mogollon sites and Kiatuthlanna (south of Allantown, east of the Petrified Forest), on which undue emphasis has perhaps been placed, is not reported from later horizons, and is missing even at the Forestdale pit-house site.

It seems sufficiently clear that the cultural remains of the several districts of eastern-central Arizona and southwestern New Mexico are not individual peripheral variants of San Juan Anasazi with minor local peculiarities. The deviations are of major importance, and the same ones occur consistently throughout this large area.

Nevertheless, these groups and the San Juan Anasazi all built pueblos, inhumed their dead, and polished their pottery, and clearly belong together as against the Hohokam. The obvious solution is not to use Anasazi and Pueblo (or Basket Maker-Pueblo) as synonyms, but to use "Pueblo" as the general category, and restrict the term "Anasazi" to the distinctive culture of the San Juan and adjacent districts, as I have previously suggested.[9]

Following out this line of thought, the general Puebloan pattern would be defined by: pit-houses (early) and masonry or adobe pueblos (later, from before 900 AD in some districts, and to the present); polished painted pottery, rough plain and polished and/or corrugated utility ware; inhumation of the dead; artificial cranial deformation; grooved stone axes, full-grooved globular mauls, metates, etc.; abundance of bone implements.

These traits largely contrast with the Gila-Sonora or Hohokam pattern, which is characterized by rancherias of huts instead of pit-houses and pueblos, ball courts, unpolished micaceous oxidized pottery, cremation, abundance of shell ornaments and sparseness of bone tools, absence of the grooved maul, and other distinctive traits.

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8 Bartlett, *Pueblo Milling Stones.*

9 Reed, *Implications of the Mogollon Concept; idem, The Distinctive Features; idem, The Western Pueblo Archaeological Complex.*

The Anasazi, a highly distinctive and comparatively uniform complex, can be defined, within the Puebloan group, by: circular or squarish pit-houses, and surface storage structures, followed by large and small pueblos with circular kivas; black-on-white and gray corrugated pottery, plus the orangeware of the western subdivision; predominance of flexed inhumation; lambdoid deformation; full-grooved or crudely notched axes. The Anasazi complex can be placed geographically as extending from the upper Rio Grande across the plateau to the Virgin River in southwestern Utah and southern Nevada, and from the Dolores River in southwestern Colorado south into the Little Colorado drainage.

The rest of the Puebloan groups are distinguished quite sharply from the San Juan Anasazi complex by: absence of circular chambers, and occurrence (in at least certain branches) of rectangular kivas; polished brownware and variety of painted pottery; vertical occipital cranial deformation; ¾-grooved axes. In Chihuahua and in the Mimbres branch of southwestern New Mexico, burials continued to be flexed, but in eastern-central Arizona—the entire Salt River drainage and adjacent districts—extended inhumation is the standard rite in the later horizons.

On the basis of pottery, the southern brownware groups or non-Anasazi pueblos extend from the lower Pecos River across southern New Mexico and across the entire Gila-Salt-Verde drainage to western Arizona, and from Chihuahua to the Little Colorado. Type of kiva and style of cranial deformation, wherever known, support this. The ¾-grooved axe has essentially the same distribution in the Southwest. The apparent interrelationship of cultural traditions in thise whole area, suggested by trait-distributions, is strengthened by the observable continuities from early to late horizons.

The early Mogollon complex in southwestern New Mexico admittedly developed into classic Mimbres, which is characterized, among other things, by rectangular kivas, brown utility ware, and ¾-grooved axes. The supposed overwhelming Anasazi domination of classic Mimbres actually consists in (1) the building of large surface structures of many contiguous rooms; and (2) black-on-white pottery.

The Mimbres villages are not close to Anasazi pueblos in detail. For one thing, they lack the circular kiva with its standard features. Mimbres Black-on-White was oxidized in firing. Frequently it is actually red-on-white; the slip is an iron-free white clay; bowl interiors are generally dark. The full-grooved San Juan axe does not supersede the ¾-grooved Gila type in the Mimbres; gray corrugated ware and lambdoid deformation do not appear in the area. Most of the culture can be derived from early Mogollon, though both Anasazi and Hoho-

kam influences are represented. Obviously, then, classic Mimbres must be regarded as "a Mogollon culture," a Pueblo development but not Anasazi, and essentially of Mogollon derivation.

The pueblos of eastern-central Arizona are, clearly, to be distinguished from San Juan Anasazi and grouped broadly with the Mimbres and early Mogollon material of southwestern New Mexico. Rectangular kivas, polished brownware (plain red, polychrome, etc.), vertical occipital cranial deformation, and the ¾-grooved axe are shared with Mimbres. The absence of circular kivas alone sets all these developments in a group apart from San Juan Anasazi. The pottery suggests a basic connection. Both regions evidently received the ¾-grooved axe through the Hohokam.

Polished brownware and rectangular pit-houses characterize the eighth-ninth century horizons in both the Flagstaff area and the Mimbres area. This emphasizes the connection between eastern-central Arizona and Mimbres-Mogollon, and in addition provides a possible source for the rectangular kiva. In the San Juan, the circular kiva obviously and admittedly developed from the circular or, more often, square Anasazi pit-house of the period 500-900 AD.

That a similar specialization in function of semi-subterranean chambers, at the introduction of surface pueblos, took place south of the Little Colorado, where these chambers are rectangular, is not unlikely. In the Sinagua branch on the west, however, where the red pottery shares features of technique and shape with Hohokam, it appears possible that kivas were not developed, the Hohokam ball-court being taken over instead, at least in the Verde.[10]

Cultural horizons of the San Juan widely different in details have been combined under the single term "Anasazi" because of the known sequential development. Yet fourteenth-century material at Chaves Pass and in the Verde is less different in many ways from the eighth-ninth century horizon of the Flagstaff area than is the thirteenth-century material of the San Juan from its known antecedents of periods between 450 and 850 AD.

No good reason is apparent for hesitating to use the term "Mogollon" for late developments as well as early manifestations, to discard the notion that Anasazi influence everywhere—or anywhere—overwhelmed and submerged Mogollon about 1000 AD or at any other time, and to define Mogollon in broad terms as comprising the polished-brownware groups of the general Puebloan pattern.

If "Modified Basket Maker" in the Anasazi sequence, with pit-houses, undeformed skulls, and unslipped, unpolished, uncorrugated pottery can be grouped

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10 Schroeder, *Cultural Implications of Ball Courts.*

with Mesa Verde and Kayenta Pueblo III, the polished-brownware groups surely can be legitimately comprehended under a single term. If such use of "Mogollon" is unacceptable, I suggest the use of "Brownware Pueblo," or Mera's original "Southern Brownware Complex."[11]

Within this general "Mogollon" or "Southern Brownware" Puebloan group, the Mimbres branch is distinguished by its technically and esthetically unusual painted pottery; and also by the persistence, as in the Anasazi, of flexed burial. To the eastward in southern New Mexico, flexed burial continues throughout the Jornada branch, and axes are full-grooved as often as ¾-grooved. Little is known about cranial deformation, but pottery and architecture ally this group with Mogollon-Mimbres. In Chihuahua, similarly, flexed burial persists, in late horizons with pottery of Mogollon tradition and with ¾-grooved axes.

The pueblos of eastern-central Arizona are distinguished especially sharply, then, by the practice of extended inhumation. The groups or sites in which extended inhumation is characteristic, as well as vertical occipital deformation, rectangular kivas (or none), ¾-grooved axes, and unpainted pottery of Mogollon type (polished brownware), may be referred to temporarily as "Western Pueblo." The archaeological term "Western Pueblo" then includes the following branches of the Gladwin-Colton classification, occupying the area of eastern and central Arizona as outlined above: Sinagua, Winslow, Cibola (including the Forestdale and Salado branches and other local sequences).

In the districts surrounding this eastern-central Arizona region, "Western Pueblo" relationship is clearly observable. The Upper Gila material or Tularosa culture is variously referred to Cibola and Salado branches, and seems to fall in this general group. The Salado material which spread down the Salt into the Tonto basin in the twelfth or thirteenth century, and on into the Lower Gila about 1300, importantly affecting the Hohokam area, is very similar to Sinagua and Cibola.

The Prescott branch, west of the middle Verde, is "Western Pueblo," save for its pottery of Hohokam affiliation. This group is characterized by extended inhumation, rectangular possible ceremonial (?) rooms, ¾-grooved axe, and many minor traits of Pueblo (Sinagua) rather than Gila-Sonora (Hohokam or Patayan) affiliation.

Finally, about 1300, "Western Pueblo" influence became strong, as discussed below, in districts to the north which had been previously Anasazi.

In sum, it would appear, if Southwestern prehistory is viewed objectively, without devotion to either the "San Juan" hypothesis or the "Mogollon-is-purely-

11 Mera, *Observations*.

basic-and-early" hypothesis, and without great concern—so far as general cultural classification is concerned—about ultimate origins of traits, that the forested mountain zone of eastern-central Arizona was actually central rather than peripheral; that all along the Mogollon Rim country, from the Mimbres to Flagstaff, development from early brownware-and-pit-house horizons, roughly contemporary with, or even earlier than, Basket Maker III and Pueblo I of the Anasazi, led to rich and varied cultural manifestations of the thirteenth and fourteenth centuries which are as distinct from, and as broadly comparable to, Pueblo III in the San Juan as is the case between earlier Mogollon and developmental phases of Anasazi.

The "Western Pueblo" archaeological cultural complex shows both syncretism and invention. Masonry pueblos are generally thought to be of Anasazi origin. The probable Mogollon derivation of rectangular kivas as well as of "Western Pueblo" unpainted pottery has been indicated above. The making of black-on-white pottery was learned from the Anasazi; the origins of black-on-red and of polychromy may also be northern. Vertical occipital cranial deformation goes clear back to the Pine Lawn phase (the SU site near Reserve in southwestern New Mexico). The ¾-grooved axe is characteristic of western Mexico, as far south as Michoacan, and undoubtedly reached the Hohokam from Sonora and the several Mogollon groups from the Hohokam.

Extended inhumation, however, may possibly be a Western Pueblo development. Extended burial occurs only very rarely in early Anasazi periods—a few instances reported from Basket Maker III, and a few from Pueblo I. It does not occur at all at Forestdale. In the Mimbres branch, an extended burial at the SU site is possibly the earliest known instance of the rite in the Southwest. In the Mogollon groups, the earliest known occurrence as a standard practice is in the eighth-ninth century horizon of the Sinagua branch. Outside the Southwest, the nearest reported occurrence of extended inhumation is in Sinaloa—associated, it is interesting to note, but naturally enough, with polished redware and ¾-grooved axes.[12]

Use of white with black in decoration of redware is evidently a Cibola-branch development, and there may be other minor traits which appear first among Western Pueblo groups.

The "Western Pueblo" archaeological complex appears as especially significant in the later history of the Southwest.

By about 1300, the Mimbres, the Flagstaff area, and the entire San Juan were abandoned, whether to Apaches or because of arroyo-cutting, or due to both drought and enemy pressure combined. The Chaco Canyon pueblos, so prominent

12 Ekholm, *Excavations at Guasave.*

in the Chaco period (Pueblo II or IIIA, about 950-1150) had declined long before, and apparently had been only sparsely occupied in the Mesa Verde period (1150-1300). The Anasazi occupation of the upper Rio Grande increased greatly while the San Juan was being abandoned.

Black-on-white pottery of Chaco affiliation characterizes the upper Rio Grande generally until after 1300, Taos to perhaps 1400, and Jemez into historic times, until after 1700. Black-on-white pottery of Mesa Verde derivation is dominant in the middle upper Rio Grande area in the early fourteenth century. Other traits of San Juan Anasazi type are also to be found in the Rio Grande.

There can be little doubt that the eastern Anasazi of the upper San Juan, the Chaco-Mesa Verde area, moved to the upper Rio Grande, already an area of Anasazi culture. The western Anasazi of the Tsegi or Kayenta district undoubtedly moved in to Hopi at about 1300; the people of the Canyon de Chelly region, of Mesa Verde culture, probably also went to Hopi at the same time.

In the early fourteenth century, then, the San Juan lay abandoned and the Anasazi were concentrated primarily on the upper Rio Grande, with black-on-white pottery, full-grooved axes, circular kivas, flexed burial. The pueblos of eastern-central Arizona were at their height. The White Mountains area of the upper Salt-upper Little Colorado was heavily populated, and distinguished by polychrome types of pottery, redware decorated with black (glaze) and white paints. The Sinagua people of the Verde and Chaves Pass (whither the people of the Flagstaff area may have gone at 1300) were receiving various polychromes by trade, but still making only plain polished red pottery, apparently.

The "Western Pueblo" complex expanded as the Anasazi shrank. To the south, the Sinagua branch—with extended inhumation, vertical occipital cranial deformation, polished redware, and the ¾-grooved axe—spread into the Gila-lower Salt area, previously purely Hohokam.[13] Here and along the middle Gila, the "Salado" material is likewise Western Pueblo, deriving specifically from the Cibola group. To the north, "Western Pueblo" traits largely superseded the corresponding Anasazi diagnostics in the northern Little Colorado, the Hopi, and Zuñi districts. Continuation of flexure of inhumations is the major exception, an Anasazi persistence.

Prior to the thirteenth century, the culture of the Hopi district was Anasazi in type. In the thirteenth century, however, Hopi pottery was mainly orange, rectangular kivas appeared alongside D-shaped and round ones, axes included ¾-grooved as well as full-grooved. In the fourteenth century, and since, Hopi pottery has been oxidized (yellow), kivas rectangular, stone axes ¾-grooved. Ex-

13 Schroeder, *Did the Sinagua of the Verde Valley Settle?*

tended burials as well as flexed occurred in the fourteenth and fifteenth centuries.

Sites in and near the Zuñi district antedating the thirteenth century are primarily or basically Anasazi—circular pit-houses and kivas, gray corrugated pottery, flexed burial; painted pottery largely black-on-white, with black-on-red in some quantity; axes of both ¾-grooved and full-grooved types; lambdoid deformation occurring.

In the fourteenth century, and since, Zuñi kivas have been rectangular; Zuñi pottery has been polychrome redware (glaze and/or matt paints, on red, orange, and white slips). Cranial deformation, when practised, was of the vertical occipital type, not lambdoid, in Pueblo IV. The ¾-grooved axe was found at Hawikuh, and extended as well as flexed inhumations occurred.

The change from Anasazi to "Western Pueblo" in the material culture remains of the Hopi area may be ascribed to influence from the Little Colorado generally,[14] and perhaps to migration from the Winslow area (Homolovi) in particular. Not only oxidized pottery and ¾-grooved axes, but also extended inhumation resembling in detail the Sinagua rite[15] characterize Homolovi in this general period. In the Zuñi country, evidently the Cibola branch spread north from the White Mountains area, bringing redware decorated in black glaze and white—from which the Zuñi ceramic sequence developed along its own lines—and other "Western Pueblo" traits, such as the rectangular kiva.

The archaeology of the Acoma district is not at all well known, but glaze-painted oxidized pottery evidently became established there in the fourteenth century; modern Acoma kivas are, as at Zuñi and the Hopi towns, rectangular (Acoma and Zuñi kivas are incorporated in house-blocks; Hopi kivas are detached and subterranean). Earlier sites in the Acoma district—before and around 1300—are San Juan Anasazi, or Chaco-Mesa Verde in type. Evidently a western influence spread on east from Zuñi to Acoma at the beginning of the fourteenth century.

Manufacture of glaze-painted black-on-red pottery became established in the upper Rio Grande toward the middle of the fourteenth century. Vertical occipital deformation is predominant in the Rio Grande from the fourteenth century on, so far as known. Burials continued flexed, and circular kivas have persisted to the present day in the Rio Grande, so that Anasazi tradition remained strong. The glaze-paint sequence, from around 1350 to 1700, was paralleled by Biscuit-ware and by Jemez Black-on-White.

Another "Mogollon" or "Western" trait, of ultimate Mexican origin and

14 Brew, *The First Two Seasons at Awatovi*, p. 134.
15 Fewkes, *Two Summers' Work*, p. 32.

probably of Hohokam transmission, may perhaps also have entered along with the oxidized pottery. The finely made spiral-grooved Rio Grande axe appeared with Glaze A.[16] Square kivas in the Rio Grande, furthermore, seem to occur after the appearance of oxidized pottery and finely made axes.[17]

The redware is certainly introduced from the west, no doubt by actual immigration of people. Square kivas may have been. The type of deformation may also have come from the west, as it evidently supersedes, at about the same time, the lambdoid style characteristic of San Juan Anasazi (but could be ascribed to the Mesa Verde immigration). Consequently, it seems not impossible that the spiral-grooved Rio Grande axe—a true polished stone implement, worked from hard stone (fibrolite, now more often known as sillimanite), not a utilized river-pebble—was ultimately inspired by the equally finely made Gila-type or ¾-grooved axe, although retaining the wrapped haft. A few examples have been found in the upper Rio Grande area of axes with an incomplete groove, encircling only three sides of the axe; but they are rough specimens otherwise of San Juan type, and not true Gila-type axes.[18]

Evidently the expansion of "Western Pueblo" culture in the thirteenth and fourteenth centuries, into the Gila and the northern Little Colorado, and to Acoma, sent an offshoot on across to the Rio Grande.[19] Glaze-paint redware, probably square kivas, perhaps the making of fine polished axes, and possibly vertical occipital cranial deformation, seem to have been transmitted and taken over.

Ethnographically, Hopi, Zuñi, and Acoma are the "typical" or Western Pueblos; the Rio Grande pueblos, especially Taos, differ in many features of society and religion. Matrilineal clans are dominant in Hopi and Zuñi social organization, significant in Acoma and the Rio Grande Keres and Jemez, unimportant in the Tewa and Tiwa, lacking at Taos. Moieties are prominent in the Rio Grande pueblos, lacking at Acoma, Zuñi, and Hopi.

A possible correlation seems indicated between ethnographic and archaeological Western Pueblo complexes, linked by the rectangular kiva; and between the Anasazi of the eastern San Juan and the Tanoans of the Rio Grande, suggested by the late survival of San Juan Anasazi traits in the latter area. The moiety kivas of the Rio Grande might be connected with the pairs of great kivas occurring in the Anasazi domain, although these prehistoric "superceremonial" chambers

16 Kidder, *The Artifacts of Pecos*, pp. 50-53.

17 Tichy, *The Kivas of Paako and Kuaua.*

18 Reiter, *The Jemez Pueblo of Unshagi*, vol. 2, p. 163, and specimen at Museum of New Mexico from glaze association in Frijoles Canyon.

19 Reed, *Sources of Upper Rio Grande Culture.*

seem to occur also singly in a few cases. The fact that large round kivas are particularly characteristic of the modern Rio Grande Keresans may be explained by postulating that the Keresans took over the Tanoan moieties as a ceremonial organization, while retaining, and diffusing to Tanoans, their basic kinship system of matrilineal clans.[20]

These changes no doubt took place quite gradually and through intermarriage between matrilocal and patrilocal groups:

> rules of descent, forms of familial and kin groupings, and kinship systems, under conditions of contact with other cultures, do not ordinarily change by direct diffusion but rather by a process of internal readjustment to altered conditions of life,[21] [and] an alteration in the prevailing rule of residence is the point of departure for significant changes in social organization . . . [which] can disturb the equilibrium of a relatively stable social system and initiate a series of internal readjustments.[22]

Very possibly such mutual influence and interchange between Tanoans and Keresans in the Rio Grande would be the reason why for the Tanoan stock, alone among linguistic groupings reviewed by Murdock in "A Technique of Historical Reconstruction," the indications are contradictory and inconsistent as to original pattern.[23]

The tentative correlation of Tanoans with eastern Anasazi (Chaco-Mesa Verde) and of Keresans with the west, rather than of Keres with any Chaco or Mesa Verde group, is indicated by the Western Pueblo affiliation of Acoma. That the Keresans migrated into the upper Rio Grande from the west, moving in from the Acoma-Puerco district in the second quarter of the fourteenth century, bringing glaze-paint redware, perhaps also finely-made axes and vertical deformation and square kivas, seems to be indicated as a tentative postulate or working concept despite a number of difficulties and discrepancies.

Possible correlation of Tanoan dialect groups with archaeological subdivision has been attempted,[24] but is particularly tenuous and uncertain. Another possible arrangement, more workable in some ways than the one I previously presented, is this idea: Tiwa—original Rio Grande people; Towa (Jemez)—Gallina people, Largo phase; and Tewa—both Chaco and Mesa Verde, the Southern Tewa or Tano coming in last, at 1300, bringing the Mesa Verde-Galisteo Black-on-White.

The obvious connections between the upper San Juan and the upper Rio

20 Hawley, *Keresan Patterns.*

21 Murdock, *Social Structure,* p. 199; cf. Spoehr, *Changing Kinship Systems,* pp. 176-178.

22 Murdock, *op. cit.,* p. 202; cf. Lowie, *Primitive Society, passim,* and Titiev, *The Influence of Common Residence.*

23 Murdock, *op. cit.,* Appendix A, pp. 323-352, espc. p. 351.

24 Reed, *Sources of Upper Rio Grande Culture.*

Grande, and the relationships of Hopi, Zuñi, and Acoma to the late-prehistoric pueblos of eastern-central Arizona, lead in any case to a general suggestion that the two lines of development observable in the archaeological material can be traced at least as cultural traditions into the historic period. One of these traditions is distinguished by gray corrugated pottery, full-grooved axes, circular kivas, lambdoid cranial deformation, predominance of flexed inhumation. The term Anasazi should be restricted to this group. The other major development is characterized by polished brownware (red, orange, yellow), ¾-grooved polished stone axes, only rectangular ceremonial rooms, vertical occipital deformation, and generally—in the west, in Arizona—by extended inhumation.

Anasazi influences or elements were important in Hopi and no doubt also at Zuñi and Acoma, but survived primarily in the upper Rio Grande. Elements of the Mogollon or Southern Brownware tradition became predominant in the west, and at least one or two western elements, such as glaze-paint redware and square kivas, reached the Rio Grande.

Admittedly, "it is not wise to write of battles and conquests [or peaceful immigrations] when one's only evidence consists of changing designs on the pottery or changing customs in the disposal of the dead, [or] to see political convulsions in changing vowel sounds."[25] Yet some reasonable and meaningful arrangement of archaeological data is imperative to produce the framework of history which is the objective and justification of archaeological excavating and collecting and studying and analyzing:

> science is in quest of knowledge as well as information; hence it is a form of intellectual cowardice to maintain or imply that we should stop with the accumulation of "facts" simply because their interpretation is fraught with difficulties and perils and has in the past led anthropologists to positions which have subsequently been shown to be absurd. And it is a form of intellectual naivete to believe that anthropology [or history: archaeology] could dispense with theory if it would.[26]

Archaeology, or history, is frequently called "not a science" because controlled experiment is impossible. The nearest approach we can make is through verification of hypotheses—that is, to point out that if a certain theory or hypothesis or suggestion is correct, then certain things ought to be found in certain places; and to look and see if they are there.

Several years ago, when beginning the study of which this is another segment, I "predicted," though not in writing, that the stone axes found by Fewkes at Homolovi, but not described or illustrated in his report, should be ¾-grooved.

25 Wainwright, *Problems and Policies*, p. 78.
26 Kluckhohn, *The Place of Theory*, p. 334.

A statement kindly furnished from the National Museum upon examination of the specimens revealed that they were.[27] I had thought so on the basis of the other diagnostic traits associated. It also struck me that there should be extended inhumations and ¾-grooved axes at Hawikuh. There were, Dr Hodge kindly informed me.[28] The correlation of flexed burial and lambdoid deformation in the Flagstaff region[29] I had not predicted before it struck me and I obtained confirmation; but it is such exceptionally good evidence for my "hypothesis" (which is really just a matter of pointing out known facts and applying a few labels) that I could scarcely have foreseen it.

There is no inherent or necessary connection between the practice of extended inhumation and the vertical occipital type of cranial deformation, or between ¾-grooved axes and rectangular kivas, or of any of these with red pottery. Any one of these works just as well without the others. Consequently, if they are found to occur together pretty regularly—which they are—the inference is that these traits happen to be among the customs of a certain group of people forming a distinct cultural unit or complex.

The traits specified above perhaps form what J. Eric Thompson has called a "flocculation," a group of traits which happen to be found together and which are not functionally interrelated or strictly coterminous,[30] but they are diagnostic of a cultural complex and a separate historical development and a probably more or less interrelated people. If one of these traits appears without the others, the spread of an idea to a different cultural unit (group of people) is indicated. If all or most of the diagnostic traits mentioned suddenly spread together to an area characterized previously by flexure, lambdoid deformation, full-grooved axes, circular kivas, and grayware, then actual movement of people, or at the very least a strong "cultural influence" is clearly implied. And such an "influence" has to be effected by people, somehow.

Whatever the mechanisms of spread—no doubt trade and stimulus-diffusion in some cases, actual migration of people in others—it seems clear that a rich and important syncretistic cultural tradition, which developed in the forested upland zone of central Arizona and expanded widely in late-prehistoric times to affect virtually the entire Southwest, is recognizable as distinct from the other—heretofore often thought of as the only—major cultural division of the general Puebloan pattern, the San Juan Anasazi of the northern Southwest.

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27 Letter of January 15, 1942 from Neil M. Judd.
28 Letter of December 4, 1941 from Dr F. W. Hodge.
29 Cf. Colton, *The Sinagua*, p. 296; King, *Nalakihu*, pp. 84, 148.
30 Thompson, *Dating of Certain Inscriptions*, p. 55.

Possibly less unusual and less conservative than the Anasazi, with their unique ceramic developments and highly distinctive ceremonial chambers, the "western" pueblos, drawing on Anasazi and Hohokam sources as well as a Mogollon base, developed a complex and advanced cultural pattern and seemingly contributed major elements to the historic Western Pueblos and to a lesser degree also to the upper Rio Grande.

Aside from speculation as to possible tie-ups between archaeological complexes and specific historic peoples, it appears unquestionable that the "Western Pueblo" archaeological division—essentially of ultimate Mogollon derivation—contributed at least as much as did the San Juan Anasazi group to the material cultural objects, and presumably to the intangible culture and the actual population of the Pueblo Indian settlements which were found by the Spaniards who entered the Southwest in the sixteenth century and which, in large part and with certain changes, are still occupied by the same peoples today.

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A WOODEN SPEAR OF THIRD INTERGLACIAL AGE FROM LOWER SAXONY

HALLAM L. MOVIUS, JR.

PROFESSOR JACOB-FRIESEN, Director of the Landesmuseum in Hannover, Germany, the results of whose important research on a series of Lower Palaeolithic (Acheulian) localities in the Leine valley, south of Hannover, have just been published,[1] has recently announced the discovery of a wooden spear in deposits of Third (Riss-Würm) Interglacial age.[2] This spear, which is 2.40 meters (7 feet, 10½ inches) long and made of yew (see Fig. 2), was found between the ribs of a skeleton of the extinct Straight-Tusked Elephant (*Hesperoloxodon antiquus* Falc.), a forest-living form that makes its last appearance in Europe during Third Interglacial times. Prior to this discovery, the only comparable find that had been recorded was the distal portion of a wooden spear 38.8 centimeters (15¼ inches) long from the well-known Second Interglacial deposits at Clacton-on-Sea, Essex, England.[3] The new spear, however, is very much more complete. It was found at Lehringen, a small village in the valley of the Aller River, near Verden (see Fig. 1), approximately 22 miles (35 kilometers) southeast of Bremen.

In this region there are extensive deposits of marl, which in places are up to 12.00 meters thick. This was accumulated in a series of shallow lakes or ponds that were formed in this broad flat valley during Riss-Würm (Saale-Warthe) times, and it contains an abundance of freshwater molluscs and plant remains. These demonstrate that a mild interglacial-type of climate prevailed at the time of deposition. Pollen analysis shows that in the forests of the Aller Valley, where *Hesperoloxodon antiquus* still roamed, oak and birch predominated. In addition there was poplar, hornbeam, mountain ash, hazel, willow, holly, and yew. The

1 K. H. Jacob-Friesen, *Die Altsteinzeitfunde aus dem Leinetal bei Hannover* (Veröffentlichungen der Urgeschichtlichen Sammlungen des Landesmuseums zu Hannover, bd. 10, pp. 1-133, 1949 [56 text figs., 3 pls.]).

2 K. H. Jacob-Friesen, *Grosswildjäger des Eiszeitalters in Niedersachsen* (Kosmos, heft 11, pp. 408-412, 1949).

3 S. Hazzledine Warren, *The Mesvinian Industry of Clacton-on-Sea, Essex* (Proceedings, Prehistoric Society of East Anglia, vol. 3, pp. 597-602, 1922). For illustrations of this object, see O. G. S. Crawford, *Man and his Past* (Oxford, 1921), plate facing p. 16 and pp. 14-15, fn. 2; K. P. Oakley, *Man the Tool-Maker* (Guide Book, British Museum of Natural History, Department of Geology, 1949), p. 15, fig. 5. The geology of this locality and the archaeological material from it are described by W. B. R. King and K. P. Oakley (Proceedings, Prehistoric Society of East Anglia, n. s., vol. 2, pp. 52-77, 1936) and by K. P. Oakley and Mary Leakey (*idem*, vol. 3, pt. 2, pp. 217-260, 1937).

skeleton was found at a depth of 2.00 meters in the marl during the course of excavations undertaken in the spring of 1948.

No details concerning the actual finding of the spear have yet been published, except for the fact that it lay between the ribs of the fossil elephant. Figure 2, a drawing based on the photograph that appears in Prof Jacob-Friesen's article,[4]

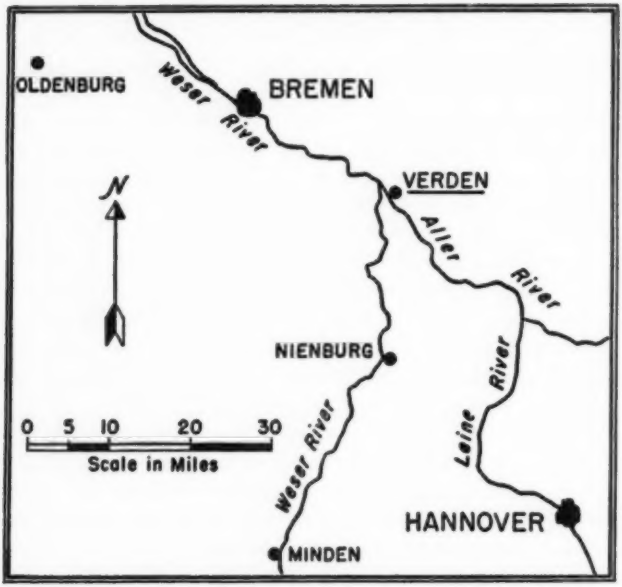

FIG. 1. Map showing the location of Verden in the Aller valley, Lower Saxony, northwestern Germany. Lehringen is a small village near Verden.

shows that the object was broken into ten fragments. In any case, it did not have a stone point, for Jacob-Friesen states that "As can be very clearly seen, the spear point has been well sharpened with stone knives, and then hardened in the fire."[5] Furthermore, the yew wood of which it is made is especially tough and hard.

As the bones of the Straight-Tusked Elephant were cleaned, approximately two dozen flint flakes came to light, but unfortunately these do not include any

4 *Grosswildjäger des Eiszeitalters in Niedersachsen*, p. 411, abb. 4.
5 *Ibid.*

clearly recognizable forms, and, with the exception of a few that have been superficially chipped on the butt-end, none of them have been worked. However, on the basis of the typology of the slightly older material from the Leinetal sites near Hannover, which is of Early Saale or Riss age, it seems very likely that Jacob-Friesen is correct in suggesting that the Lehringen find can be attributed to the Upper Acheulian. As far as Europe is concerned, it is the first find of a prehistoric elephant-hunters' site that has thus far been reported.[6]

The remainder of Jacob-Friesen's article is concerned with a discussion of how elephant hunting may have been practised during Palaeolithic times. First

FIG. 2. The wooden spear 2.40 meters (7 feet, 10½ inches) long and made of yew wood found in Third Interglacial deposits at Lehringen, near Verden, Lower Saxony.

of all he states that, on the basis of analogy with living animals, various German experts on elephants do not consider it probable that the skin of this beast was too thick and tough to prevent penetration by a spear made of yew wood, the tip of which had been sharpened in the fire. As to the actual method, the author cites a book by Ernst Zwilling, *Unvergessenes Kamerun*, in which an elephant hunt performed by the Pygmies of the Cameroon jungle is described. After sighting a single elephant feeding or sleeping in the thick forest, the hunter very stealthily closes in on his quarry, until he is close enough to thrust his 2.10 meters long spear with both hands into the body of the animal. The elephant, blind with pain, charges through the dense underbrush, the spear penetrating deeper and

6 A temporary mammoth-hunters' camp was found during 1896 below 3.50 meters of Upper Pleistocene loess in the outskirts of Tomsk, a city on the Ob River in western Siberia. According to N. F. Kashchenko (Izvestiia Imperial Akademie Nauk, vol. 5, no. 1, 1896, p. 31; Zapiski Imperial Akademie Nauk, 8th ser., vol. 11, no. 7, pp. 1-60, 1901; cf. also S. K. Kusnezow (Mitteilungen der Anthropologisches Gesellschaft in Wien, bd. 26, pp. 186-191, 1896), who discovered this locality, the broken and splintered bones of a young mammoth covered a total area of 8.00 meters by 4.00 meters. Many of them exhibited superficially cut and scraped surfaces. A hearth approximately one centimeter thick indicates that the hunters killed and ate the animal on the spot. As in the case of the Lehringen find, very few of the associated stone implements had been worked. Of a total of nearly 200 blades, flakes and nuclei, less than 25 percent exhibited any traces of use whatsoever. Apparently local rocks were collected along the river banks, and the knapping was done on the spot. However, this does not explain the virtual absence of retouched forms.

deeper into its entrails. Finally it dies several days later, the bloody trail having been easily followed by the hunters. According to Zwilling, a Pygmy is rarely killed as the result of one of these hunts, since the elephant normally tries to run away when suddenly surprised by the painful thrust of the spear.

Possibly the Lehringen elephant was stalked and speared in this manner by a group of Palaeolithic hunters during Third Interglacial times. The beast then ran away and finally lay down to die in the lake muds of the Aller valley. That the hunters cut the flesh off the animal at this spot is indicated by the comparatively small and unretouched flint flakes found among the bones. Not only does this find suggest an interesting picture of prehistoric hunting methods, based on a modern ethnological parallel, but also it serves to emphasize the fact that wooden implements and weapons must have been commonly used by our Palaeolithic forbears. Indeed they may well have played an even more dominant role in the daily economy of the hunting peoples of the Old Stone Age than did tools fashioned of stone. The answer to this question will only be provided if and when an actual occupation site is found in interglacial peats or lake deposits, such as those of Lehringen, where organic remains have been preserved. In the meantime, this locality should be carefully watched for just such a site. After all the Third Interglacial elephant hunters may have been living on the shores of the lake in the muds of which their quarry finally died.

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STUDIES IN AFRICAN LINGUISTIC CLASSIFICATION:
V. THE EASTERN SUDANIC FAMILY

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THE EASTERN SUDANIC family of languages consists of the following seven branches: 1. Nubian; 2. Beir-Didinga; 3. Barea; 4. Tabi; 5. Merarit; 6. Dagu; 7. Southern branch: a. Nilotic, consisting of α. Burun, β. Luo group (Shilluk, Anuak, Bor, Acholi, Lango, Alur, Chopi, Jaluo, Luo, Budama), γ. Dinka-Nuer; b. Great Lakes ("Nilo-Hamitic"), consisting of α. Nandi-Suk, β. Bari, Fajulu, Nyangbara, Kakwa, Masai, Teso, Turkana, Topotha, Karamojo, Latuka.

Before attempting to establish the validity of the general Eastern Sudanic grouping, I shall first seek to prove the reality of the Southern branch as listed above, that is, to demonstrate the relationship between the so-called Nilo-Hamitic and the Nilotic languages. As the traditional name Nilo-Hamitic indicates, these languages have often been considered either "Hamitic" or fundamentally Hamitic-influenced.[1] The various versions of the Nilo-Hamitic hypotheses are considered below. In the following discussion, this traditional but misleading term has been replaced by the geographical designation, Great Lakes languages.

Lexical comparison of the Great Lakes and the Nilotic languages reveal vocabulary resemblances in fundamental noun, adjective, and verb stems that are so obvious and extensive in number that it would be pointless to enumerate them. In a list of common nouns, resemblances between two groups would run over fifty percent.[2] Nor does anyone take the impossible position of denying their existence. They are simply discounted by many authors in favor of supposed Hamitic structural features in morphology. In the exposition which follows, I shall attempt to show that there are many highly specific points of resemblance to the Nilotic languages in morphology as well as in vocabulary, while there is little that points in the direction of Hamitic-Semitic.

1. *Personal pronouns.* There is practical identity in the set of singular pronouns used as subjects of verbs in the Nilotic and the Great Lakes languages.

1 As has been indicated in the fourth article of this series, the term "Hamitic" does not refer to any valid linguistic entity. I continue to use it in this article, interchangeably with Hamito-Semitic, in reproducing the arguments of those who employ this term.

2 Thus, of thirty-one common nouns of the chief Nilotic and Great Lakes languages listed in the *Distribution of the Nilotic and Nilo-Hamitic Languages of Africa* by M. A. Bryan and A. N. Tucker (London, 1948) and not chosen for purpose of such a comparison, nineteen are clearly cognate.

Thus Masai among the Great Lakes languages with *a-mat* 'I drink', *i-mat* 'thou drinkest', *e-mat* 'he, she, it drinks' parallels Lango, a Nilotic language, which has *a-mato* 'I drink', *i-mato* 'thou drinkest', *e-mato* or *o-mato* 'he, she, it drinks'. Forms similar to these are found throughout both groups of languages. The same sequence *a, i, e* is found in the independent singular pronouns of both groups with the addition of an *n* element. In the meaning 'I' we may compare Great Lakes Nandi, Suk *ane,* Latuka *ani,* Karamojo *ano,* Kakwa *an* with Nilotic Shilluk *an,* Anuak *ana,* Dinka, Nuer *yan,* Alur, Nyifwa *ani.* In the second person, we have Great Lakes Nandi *inye,* Suk *nyi,* Turkana *ion,* Nilotic Shilluk *in,* Dinka *yin,* Anuak *ini.* For the third person singular independent pronoun, the Great Lakes languages generally employ masculine and feminine demonstratives, but Masai *ninye,* Nandi *ine* still remind us of Anuak *ena,* Dinka *yen,* Shilluk *en,* Nyifwa *ino.*

In the plural pronouns the Nilotic languages use the same set both in independent use and as verb subjects. These forms are in general comparable to the Great Lakes independent plural pronouns. There is a general tendency in both groups to use a *-k* plural suffix in these forms, carried out most completely in the Great Lakes Nandi *aček* 'we', *okwek* 'you', *iček* (<*ikek*) 'they' and the Nilotic Dinka *ɣok* 'we', *wek* 'you', *kek* 'they'. There is considerable diversity even between relatively closely related languages in the plural pronouns. We may reconstruct the first person plural pronouns as **kɔ* on the basis of the Nilotic Nuer *-kɔ* verbal, *kɔn* independent and the Great Lakes Latuka *xɔxɔi* (<*kɔkɔi*), Karamojo *eko-k,* Turkana *xo-si* (<*ko-si*). In the second plural, Nilotic Nuer *ye,* Dinka *we-k* and Great Lakes Teso *ye-s,* Turkana *e-θ* seem to point to **we* or **ye.* The third person plural pronouns are clearly **ke* as evidenced by Nuer *kɛ,* Dinka *ke-k,* Great Lakes Bari *se* (<*ke*), Teso *ke-s,* Turkana *ke-θ,* Nandi *iče-k* (<*ike-k*).

For personal possessive pronouns both groups suffix forms to the noun which are generally identical with the dependent verbal subject pronouns which have already been discussed. However, Dinka and Nuer in the Nilotic group show an interesting divergence in that *-u* appears in the second person singular in place of the expected *-i.* We likewise find *u* in Acholi, suffixed to kinship forms. This same *u* occurs in combination with other elements in Nandi—*-ŋuŋ* after singular nouns and *-kuk* after plurals. Likewise in Turkana we have it as *-o* in the form *-kon* 'thy' where *-kin* would have been expected in the series *kaŋ* 'my', *kon* 'thy', *keŋ* 'his, hers, its'. Another divergence is Dinka *-ya* 'my' which seems to correspond to Nandi *-yo:,* Bari *-io.*

To compare with this mass of detailed resemblances between the Great Lakes and the Nilotic personal pronouns, Hamito-Semitic responds with fortuitous re-

semblances of Nandi *ane* etc. to Semitic *ʔana:*, Galla *ani*, 'I'. As has been seen, these Great Lakes forms consist of *a-*, the dependent pronoun, plus *-n*, *-ne*. On the other hand the Hamito-Semitic forms display a pronominal base *ʔan* seen in Semitic *ʔan-ta* 'thou' etc. so that *ʔana:* has usually been analyzed as *ʔan* plus *a:*. We have to do then with an accidental convergence in a single form. In general the Hamito-Semitic personal pronouns are totally different from those of the Great Lakes languages.

2. *Interrogative pronouns.* In the forms for the interrogative pronouns, the Nilotic and Great Lakes languages show close agreement.

'who?': Great Lakes—Turkana *ŋai*, Latuka *ŋai*, Masai *aŋae*, Bari *ŋa*. Nilotic—Dinka, Nuer, Anuak *ŋa*.

'what?': Great Lakes—Bari, *ŋyo*, Turkana *ño*, Latuka *naiñu*, Masai *aiño*. Nilotic—Nuer *ŋu*, Dinka *ŋo*, Shilluk *aŋu*. These forms exhibit no similarity to the Hamito-Semitic forms.

3. *Noun plurals.* Both the Nilotic and the Great Lakes languages show considerable variety of noun plural formation. There are important points of detailed agreement. The ubiquitous *k* plural appears in nouns in both groups. It particularly characterizes mass nouns, that is liquids and other indefinitely divisible substances, thus, Nilotic—Shilluk *fik* 'water', *ča:k* 'milk'; Great Lakes—Nandi *pek* 'water', Masai *iŋ-gulak* 'urine', *iŋ-amulak* 'saliva', Bari *kamulak* 'saliva'. In both groups an *-i* plural is frequently found. Another formation is the dropping of a final *o*, *u* of the singular to form the plural: Nilotic—Shilluk *gyeno/gyen* 'hen', *falo/fal* 'knife'; Great Lakes—Masai *ol-kurto/il-kurt* 'caterpillar', *en-duduño/in-duduño* 'heel', Teso *e-moru/i-mor* 'hill'. Most important of all, however, because of their high degree of specificity are the plurals involving final consonant replacements combined with internal change in two common words. The variation to be seen in the singular and plural forms of the word for 'cow' in the Nilotic Southern Burun *deŋ/duk*, Shilluk *deaŋ/dok* appear also in the Great Lakes languages as Teso *ai-teŋ/ai-tuk*, Masai *eŋ-gi-teŋ/iŋ-gi-šuk*, Bari *ki-teŋ/ki-suk*.[3] This alternation reappears in most of the Eastern Sudanic languages. In the word for 'eye', the alternation *waŋ/ñin* found in the Nilotic Shilluk has its counterpart in Great Lakes Bari *k-ɔŋɛ/k-ɔñen*, Nandi *k-ɔŋ/k-ɔñan* etc. A further similarity in plural formation in the two groups is the occurrence of

3 Masai *gi-* and Bari *ki-* (*g-* and *k-* respectively before vowels) are prefixes frequently used before feminine nouns. This element occurs in other Eastern Sudanic languages, Nubian has a *k-* prefix in related words and it is found in Dagu in the pair *arei* 'father-in-law' *karei* 'mother-*in-law*'. It may well occur in other instances in Dagu since our knowledge of this language is very limited. In Nubian there is no direct proof that it is feminine since the language lacks grammatical gender.

-t plurals in Nilotic, e. g. Shilluk *yei/yat* 'boat' as compared with Great Lakes plurals in *-ta, -to, -ti*. Paralleling the Great Lakes *-n* plurals, we have Nuer *-ni* and nasal replacements as in Mughaja (Burun) *kak/kaŋ* 'snake'; Shilluk *kwač/kwañi* 'leopard', *aŋado/aŋani* 'breast-bone'.

The only real point of resemblance between the Hamito-Semitic languages and the Great Lakes languages in plural formation is the occurrence of *-n* plurals in both. Perhaps also the *-t* plurals might be compared to *-ota* of Galla and the Semitic feminine plurals in *-a:t*. The *-k* plural so common in the Nilotic and Great Lakes languages is non-existent in Hamito-Semitic except for the *-ki* plural of Agau, a Cushite language, and this is known to be a secondary formation from *ki* 'all'. Whatever the truth about this particular formation, no one would posit a *-k* plural for Hamito-Semitic. The characteristic partial reduplications of the Cushite and Chad Hamito-Semitic groups do not occur in the Great Lakes languages nor internal change to *a*. Considering the variety of plural forms found in both Hamito-Semitic and the Great Lakes languages, the instances cited need be no more than accidental convergences.[4]

4. *Demonstratives and Relatives.* The alternation *n* singular /*k* plural displayed in Shilluk *an/ak* 'this', 'these', Acholi *è:nò/è:gò* (same meaning), is found in Nandi among the Great Lakes languages in the forms *nu/ču* (<*ku*) 'this', 'these', and the use of singular *ne,* plural *če* (<*ke*) as a relative. The Bari adjectival form *rɔn/rɔk* 'bad' also seems to belong here. The fact that the Nandi forms are prefixed while the Nilotic ones are suffixed is not too remarkable. In the Romance languages the definite article which derives from a Latin demonstrative is suffixed in Romanian and prefixed in the western Romance languages. It is likewise probable that the *ti* plural demonstrative and relative of Nuer in the Nilotic group is cognate with the *ti* possessive and relative of Bari employed after plural nouns. None of these forms occur in Hamito-Semitic languages.

5. *The copula a.* Bari, among the Great Lakes languages, employs a form *a* both as a copula connecting noun with adjective and as a formative for the past tense. In Shilluk *a* functions in precisely the same two uses. Elsewhere in the Nilotic languages *a* functions as a copula and a general tense formative (Dinka, Nuer). This *a* reappears frequently in other Eastern Sudanic groups (see the entries under *to be* in the comparative word-list). There is nothing to parallel this in Hamito-Semitic.

The detailed similarities in morphology just cited, in conjunction with the

4 How treacherous merely a few resemblances may be when a large number of plural types are involved is shown by the fact that the language of the Yenisei Ostyak in remote Siberia has two common Hamitic-Semitic plurals, an *n* suffix and internal change to *a*.

large number of vocabulary items common to the two groups seem sufficient to prove the genetic relationship of the Nilotic and the Great Lakes or so-called Nilo-Hamitic languages beyond any reasonable doubt.

If this is so, the reader may wonder why the Great Lakes languages have been usually called Nilo-Hamitic. As a matter of fact, the term Nilo-Hamitic has been used by different writers with widely varying meanings. It seems to have been all things to all men. To some, for example Bernhard Struck who accepted Meinhof's thesis that these languages were Hamitic pure and simple, the term seems to have meant merely Hamitic languages in the Nilotic area. He states: "Besides, the term Nilo-Hamitic indicates in a very appropriate manner the Hamites inhabiting the Nile valley in a north-south direction."[5] This is evidently C. G. Seligman's view, though he correlates the linguistic term Nilo-Hamitic with a racial designation Half-Hamite, indicating Negro admixture. A different view is that of D. Westermann, who groups together the Nilotic and Great Lakes languages under the term Nilotic but calls the northern division (i. e. Nilotic as used in this article) Niloto-Sudanic and the Southern Great Lakes division Niloto-Hamitic. He remarks:

> So the line between Niloto-Sudanic and Niloto-Hamitic is not easy to define; they all have components of Sudanic and Hamitic origin, only that in some cases the first is prevalent, in others the latter.[6]

Still another nuance of opinion is that of G. W. Murray, who extends the term Niloto-Hamitic to Nubian also. (We have seen that Nubian is an Eastern Sudanic language.) After stating that in Nilotic, Bari, Masai, and Nubian we have "four nearly allied dialects" we are informed that

> at a later period Hamito-Semitic influences . . . permeated three of the groups, Nubian, Bari, and Masai, which we therefore call Niloto-Hamitic, and to a slight extent affected the Shilluk group also.[7]

To this Hamito-Semitic influence is ascribed the plural suffixes, some of the verbal derivative affixes, the gender system, and other complex morphological features of the Great Lakes languages.

Recently we have the view of A. N. Tucker who states that the Nilo-Hamitic languages are defined by three characteristics:

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5 B. Struck, "Die Sprache der Tatoga und Irakuleute," p. 119 fn. 4 in F. R. Jaeger, *Das Hochland der Riesenkrater* (Berlin, 1911).

6 D. Westermann, *The Shilluk People* (Berlin, 1912), p. 33.

7 G. W. Murray, *The Nilotic Languages, a Comparative Essay* (Journal, Royal Anthropological Institute, vol. 50, pp. 327-368, 1920), p. 360.

1. a large common vocabulary of Nilotic stems;
2. a large common vocabulary of non-Nilotic stems;
3. a large common vocabulary of Hamitic-like prefixes and suffixes.[8]

It can be seen that these opinions are of two main types: those which assert that the Great Lakes languages are Hamitic and those according to which they are basically Nilotic but have undergone Hamitic structural influence to the point where they must be considered a kind of linguistic hybrid.

The first point of view is most strongly represented by Carl Meinhof, the only writer who has sought to present actual proof of the Hamitic connections of these languages. In his volume *Die Sprachen der Hamiten*, one chapter is devoted to the Masai language as representative of the Great Lakes languages.[9] A perusal of this chapter is sufficient to show the weakness of the case Meinhof tries to present. In this work the section under each language called *Weitverbreiteste Bildungselemente* always provides the key to the validity of the Hamitic hypothesis with regard to the language under discussion. Where languages are actually Hamito-Semitic, as in the case of Hausa, a wealth of concrete forms, plural formatives, verbal derivate affixes, whole conjugational paradigms, can be cited. Where the case is extremely weak, as in the instance of Fulani, this section becomes very meager. This portion of the chapter on the Masai language consists of six short paragraphs. In the first Meinhof adduces the masculine and feminine articles of Masai and relates them to hypothetical Fulani forms for the personal and impersonal respectively. We have seen that Fulani is not Hamitic and further, that the Fulani prefix system has been misinterpreted by Meinhof.[10] The Masai *-in* plurals, which I have discussed above, are also mentioned in this paragraph. The second statement compares the Masai verbal suffix *-u* meaning 'motion hither' with Hausa *-o:* in the same meaning. Now *-u* in Masai is clearly a development of the *-un* of related languages. Indeed *-un* reappears in Masai whenever a vowel follows and must be considered the fundamental form on the basis even of the Masai data. The third paragraph concerns the *-u* future and imperative and again involves comparison only with Fulani which has been shown not to be Hamitic. The fourth item involves a comparison only with Hottentot, which it is now generally agreed is non-Hamitic and whose precise status I shall attempt to show in a subsequent article of this series. Finally the Masai formation of multiples of ten by a suffix *-tam* is compared to the similar construction in Somali.

8 *Distribution of the Nilotic and Nilo-Hamitic Languages of Africa* (London, 1948), p. 48.

9 Hamburg, 1912, pp. 211-225.

10 See Joseph H. Greenberg, *The Classification of Fulani* (Southwestern Journal of Anthropology, vol. 5, pp. 190-198, 1949), pp. 196-197.

This is clearly an instance of borrowing since Masai only resembles the neighboring Somali among the host of Hamito-Semitic languages and because it is evident that Masai has borrowed all its numerals from six upward from the Somali-Galla group of Cushite languages. The final paragraph is cryptic as to meaning. I shall quote it in full: "Compare *te-* in composition with substantives to form prepositions." It does not tell us with what to compare *te-*, nor can I discover any connection with the preceding paragraph.

To sum up, all that Meinhof is able to adduce in the way of valid specific morphological evidence is a single plural suffix, the *-n* plural, to which he might perhaps have added the *-t* plural.[11]

Meinhof seeks to strengthen his case by the citation of thirty Masai words in his comparative Hamitic vocabulary at the end of the same volume. Of these, ten are clearly borrowed from the neighboring Cushite languages, e. g. Masai *tekitam,* Somali (Cushite) *digetam* 'twenty'. Of the remaining twenty entries, one half range from the phonetically implausible to the fantastic, e. g. Masai *uni* 'three' is stated to be a development of an earlier **kuni,* for which there is no proof given. This reconstructed form is then compared with Egyptian *xmt* and Bedauye *emhay*! In other instances an ingenious Hamitic etymology is proposed when an obvious Nilotic one is at hand. Thus Masai *mat* 'to drink' is connected with Hausa *mo:da:* 'drinking vessel' and Somali *mu:d* 'brandy' when we have in Nilotic, Lango *mato,* Shilluk *ma:*do, preterit *mat*, etc., meaning 'to drink'. This leaves us with a remainder of ten etymologies which have any degree of plausibility. Altogether this is hardly more than a chance number of similarities. If one were to compare English to all the Hamito-Semitic languages, I should imagine that ten possible etymologies might be found.

Meanhof seems simply unaware of the numerous relations in fundamental vocabulary between the Nilotic and Great Lakes languages. It is this fact which has led to more complex formulations of the relationship of the Hamitic and Great Lakes languages such as that of Tucker, cited above. I believe that the morphological similarities between the Nilotic languages and the Great Lakes languages already adduced likewise exclude interpretations such as those of Westermann and Tucker.

It would seem that the basic reason why people have not accepted the rela-

11 See footnote 4 above. Another Great Lakes feature which is sometimes considered the result of Hamitic influence is the verb negative in *m-* of Masai and Nandi. However *m-* verbal negatives are just as widespread in Eastern Sudanic as in Hamito-Semitic and we need not resort to Hamito-Semitic influence as an explanation. For an enumeration of *m-* negatives in Eastern Sudanic see item 25 below in the list of morphological elements common to Eastern Sudanic languages.

tionship of the Nilotic and the Great Lakes languages is that they have appeared to be very different structurally. Friedrich Müller long ago, comparing Dinka, a Nilotic language, and Bari, a Great Lakes language, saw that they were related in spite of superficial differences. "In spite of their apparently diverse structures, we maintain that the two languages, that is Dinka and Bari, are profoundly [innerlich] related."[12] The Nilotic languages seem to be predominantly isolating, tend to monosyllabism, and employ tonal distinctions. They conform to the "Sudanic" stereotype and were at first classified by Westermann with his Sudanic. The Nilotic languages were held to represent primeval simplicity as against the agglutinating tendencies of the Great Lakes group. The complex morphology, and, above all, the presence of grammatical gender in these languages were attributed by some writers to Hamitic influence working on an undeniable Nilotic base of vocabulary.

But are the Nilotic languages indeed so simple? When thorough descriptive accounts are available, such as that by Father Crazzolara on the Nuer language, we encounter a system of noun cases and the existence of verbal derived forms.[13] For example the noun for 'tongue' in Nuer has the following forms:[13a] *lêp*, nominative singular; *léab*, genitive singular; *lèb*, locative singular; *lúf*, nominative plural, *lufnä̀*, genitive and locative plural. The Nuer word for 'mouth' is inflected thus: *θók*, nominative singular; *θwɔ̀h*, genitive singular; *θóγ*, locative singular; *θúuh*, nominative plural; *θúuhnä*, genitive and locative plural. From *mä̀ðè* 'he drinks' we have applicative *máàðè* 'he drinks something', *mä̀ä̀θé* 'he causes to drink'. If this is primeval simplicity, I prefer an uncomplicated inflectional language like classical Latin or Sanskrit!

It is clear that the predominant monosyllabism of languages like Shilluk and Nuer is historically secondary, just as in the case of Chinese or English. The intricate internal variations of languages like Nuer must be the result of changes induced by former affixes which have been dropped after influencing the root. This is the manner in which internal changes are normally known to develop where historical evidence is available. Thus the English umlaut alternatives *foot/feet* is known to have arisen from a former *-i* in the plural which modified *o:* to *e:* before it dropped. This general process is obvious from the internal

12 *Grundriss der Sprachwissenschaft* (Wien, 1876), vol. 1, part 2, p. 181.

13 J. P. Crazzolara, *Outlines of a Nuer Grammar* (Wien, 1933).

13a Because of limitations of type available, substitutions have been made in the printing of Nuer words: *ê, é, è* for correspondingly accented epsilon; the grave accent should be superimposed on inverted *c*, the acute accent on *ä*. In certain other words roman d, n, t have been substituted for corresponding fronted forms, which should appear here with inverted breve below these consonants—Editor.

evidence of Nuer itself where final *-a* has resulted in the diphthongization of a previous vowel by anticipation of the final *-a*. For example, the variation, *lɛp*, nominative / *lɛab*, genitive, in the above example, is plausibly explained by the loss of a final genitive *-a* (<**lɛpa*). This formative *-a* still occurs as a genitive in other words in Nuer. The shift *p*>*b* is the result of voicing by the former vowel that has disappeared. In Nuer unvoiced consonants never occur intervocalically. Similarly in Nuer and in Dinka, the suffixed and still present personal pronouns produce internal modifications in the root. So I would interpret the constant *raising* of the root-vowel in Dinka to form the plurals of nouns as the result of the former *-i* plural suffix found so frequently in allied languages such as Shilluk.[14] It is precisely analogous in origin to the Umlaut plurals of Germanic languages. Finally, there is the fact that two widely separate Nilotic languages, the Burun of the northeast and Alur in the south (as recorded by Czekanowski) still show final vowels in most nouns where they have been lost in other Nilotic languages. Thus we have Northern Burun *woŋgu* 'eye' where Shilluk has *woŋ, idu* for Shilluk *yit* 'ear' etc. Instead of the *-k* plurals of Shilluk, we have *-ku, -ka* which cannot be new formations since they agree with formatives in other Eastern Sudanic languages (Nubian and Barea). Likewise the singular formative *-it* in Mughaja (a Burun dialect) *gurit* 'stone' agrees with suffixes in the Great Lakes languages as well as Nubian.

It is true that the Great Lakes languages have a gender system; so have French and Chinook. The specific affixes which denote gender in the Great Lakes languages are *l* masculine, *n* feminine. They show no similarity to the gender affirmatives of the Hamito-Semitic languages. On the other hand we have in the Nilotic Shilluk, *ña* 'child', *ña-l* 'boy', and *ña-n* 'girl' with precisely the same gender-denoting elements as those of the Great Lakes languages.

Another structural feature of the Great Lakes languages which has been interpreted as an evidence of Hamito-Semitic influence is the presence of a system of verbal derivative affixes with causative reflexive, inchoative and other meanings. Here again, the afformatives of the Great Lakes languages exhibit practically no affinity to those of the Hamito-Semitic languages while a closer examination of the facts again indicates real resemblances to the Nilotic languages. The Nilotic languages, as we have seen, do have verbal derivational forms. These are partly indicated by internal changes of vowel quantity, quality, and tone in the verb root, partly also by changes of final consonants. These latter consonantal

14 This has been seen for Alur, another Nilotic language, by M. Vanneste, *Regels der Welluidenheit in de Alur-taal* (*Mahagi*) (Congo, vol. 2, pp. 49-51, 1925). Vanneste states: "'t zijn daar misschien vormen van umlaut veroorzakt door de *i, o* of *u* van der uitgang" (p. 50).

alternatives can be plausibly analyzed as the result of affixes similar to those of the Great Lakes languages. For example, in Bari and Latuka, there appears an inchoative suffix *-an* 'to become', e. g. Bari *dar-an* 'to become red'. The Nilotic Shilluk replaces a final stop by its corresponding nasal to give the same meaning, e. g. Shilluk *rač* 'bad', *reño* 'to become bad'; *nɔk* 'little', *nɔŋo* 'to become little'.[15] The final *-o* is the usual Shilluk present suffix. The case for this connection is strengthened by the appearance of the same inchoative suffix in other Eastern Sudanic languages, Nubian and Barea.

Another instance is the alternation between a final midpalatal or dental stop and its corresponding nasal found in a number of instances in Nuer and associated with motion away from and toward the speaker respectively. Thus we have Nuer *čóòθé* 'he enters away from' and *čúunè* 'he enters toward'. In the Great Lakes languages, motion away from is indicated in the Nandi subgroup of languages by *-te* while Bari has *-or, -od*. Motion towards is indicated by *-un* in Bari, Teso, and Masai. Compare also Latuka *iweta,* 'go!' *iwena,* 'come!'. The suffiix *-ki, -kin* with the dative meaning 'to do for somebody' has cognates not indeed in Nilotic, but in Didinga, another Eastern Sudanic language, which has *-eki* in the same meaning. In general, not only in the formal means of expressing these categories, but semantically, the resemblance of the Great Lakes verbal derivate system is to the Nilotic and other Eastern Sudanic languages rather than to Hamito-Semitic. The Eastern Sudanic languages show a more extensive set of meaning categories than the Hamito-Semitic. We find not only causative, reflexive, and passive as in Hamito-Semitic but such additional types as applicative, dative, inchoative, and directional. In fact, outside of the divergence in the concrete methods of the two groups of languages in expressing gender and other meaning categories, it must remain one of the higher mysteries of linguistic science how, in accordance with most versions of the Nilo-Hamitic hypothesis, the three-gender systems of the Masai and Teso could have arisen from the Hamitic two gender system, the numerous and complex Great Lakes plural formations could have derived from the simpler and less numerous methods of Hamitic languages, and how the Great Lakes system of verbal derivates with its large number of categories could have originated in the comparatively uncomplicated Hamitic system.

To sum up. The evidence of both vocabulary and morphology indicates genetic relationship of the Great Lakes and Nilotic languages. Where the Great Lakes languages border Hamitic languages of the Cushite group, there has been, not unexpectedly, a certain amount of word-borrowing which has been moderate

15 *The Shilluk People,* p. 58.

in scope. This contact situation has not significantly affected the basic structure of the Great Lakes languages.

Borrowing by the Turkana from the Somali has been particularly frequent, by the Masai and Nandi somewhat less so. It has usually been assumed *a priori*, that the borrowing must always have been in one direction, from Hamitic to Great Lakes, apparently in agreement with the general assumption of Hamitic superiority. In some instances it is possible to show that the borrowing must have been by Hamitic speakers from Great Lakes languages. In other instances, the evidence is not sufficient to decide the question. In the common Somali word *biyo, piyɔ*, 'water' (also found in a few smaller neighboring Hamitic Cushite groups), there can be little doubt of borrowing from Great Lakes or other East Sudanic languages on the basis of distributions. While isolated in Cushitic as well as in Hamito-Semitic generally, it is a common Eastern Sudanic word found in Nandi *peiyɔ*, which may be its immediate source in Somali, in Dinka *piu*, Nuer *pi*, Tabi *fi-k*, etc. Finally it is worth remarking that recent studies indicate that the Great Lakes languages resemble the Nilotic in being tonal whereas the Hamito-Semitic languages are, in general, non-tonal.

The problem of the relationships of the Great Lakes languages has been discussed at this length only because of the widely accepted opinions concerning Hamitic affiliation or influence. I am aware that the very strength of the case that can be made against this common assumption may cause the reader to react against it as a possible oversimplification on the well-known principle that where there is a great deal of smoke there must be at least some fire. I can only ask him to draw his own conclusions after comparing the evidence assembled here with Meinhof's chapter on the Masai language in *Die Sprachen der Hamiten*, which is the only systematic attempt ever made to prove the Hamitic thesis.

It is a familiar hypothesis that the Nilotic languages are related to such other languages in this area as Nuba, Barea, and Didinga.[16] The inclusion of Merarit and Dagu and the exclusion of Kunama will no doubt occasion surprise. Although our material on Merarit and Dagu—particularly the latter—is extremely scarce, a high proportion of common vocabulary resemblances to the other Eastern Sudanic languages is found. Likewise a considerable degree of morphological resemblance is apparent in spite of the scanty material. Particularly striking are the plural formations for 'cow' and 'tree', for which the comparative word lists below may be consulted. The same argument applies to Tabi, on which there is very little published material.

16 See, for example, C. Conti Rossini, *Lingue Nilotiche* (Rivista degli Studi Orientali, vol. 11, pp. 69-168, 1926).

On the other hand Kunama, which is almost always mentioned in the same breath as the neighboring Barea, but is actually very different from it, shows vocabulary resemblances only in a few common words, e. g. *aro* 'white', *na* 'who?'. In morphology a few items, the noun plural *-i*, the first person pronoun *a*, the relative *ma* suggest Eastern Sudanic affiliations. In spite of the relatively abundant documentation of this language, the points of similarity both in vocabulary and morphology are significantly fewer than in the other languages. Using Tibetan as a control, I found as many resemblances to other Eastern Sudanic languages in Tibetan as in Kunama. I have therefore rejected this relationship as unproven at the present state of our knowledge.

The evidence for the validity of the Eastern Sudanic grouping is presented here in the form of a catalogue of morphological elements followed by a comparative word-list. Some comparisons involving only the Nubian and the Southern branches (Nilotic and Great Lakes) of the Eastern Sudanic family have been omitted.[17] The rather large number of such comparisons does not, in my opinion, rest upon any special relationship of these two branches but simply reflects the more ample documentation in the case of these languages.

Eastern Sudanic Morphological Elements[18]

1. *First person singular subject pronoun.* (1a) Shilluk, *a* (object pronoun), Nuer, Dinka *a;* (1b) Masai, Turkana, Nandi, Teso, Latuka *a;* (2) Nuba *ai;* (3) Didinga *a;* (5) Tabi *a-ne* (independent pronoun); (6) Merarit *wa;* (7) Dagu *a*

2. *Second person subject pronoun—singular.* (1a) Dinka, Nuer, Acholi, Lango *i;* (1b) Masai, Nandi, Teso, Latuka *i;* (2) Nile Nubian *i-r* (independent pronoun), Midob *i-n* (independent pronoun); (3) Didinga *i;* (6) Merarit *i;* (7) Dagu *i*

3. *Second person possessive pronoun.* (1a) Dinka, Nuer, *u*, Acholi *-u* (in relationship terms); (1b) Nanda (*k*)*u*(*k*), etc.; (3) Didinga (*c*)*u*(*ni*); (5) Tabi *u*(*n*); (6) Merarit *onu*

4. *Third person demonstrative.* (2) Nubian *te-r;* (3) Didinga *či;* (4) Barea *ti;* (6) Merarit *te*

5. *n/k singular-plural alternation in demonstratives.* (1a) Shilluk *an/ak* 'this,

17 For such additional comparisons see the above-mentioned essay of G. W. Murray. Not all of his comparisons are convincing, however.

18 In this section and the following word list, the numbers refer to the various branches of the Eastern Sudanic family, as follows: (1a) Nilotic, (1b) Great Lakes, (2) Nubian, (3) Didinga, (4) Barea, (5) Tabi, (6) Merarit, (7) Dagu.

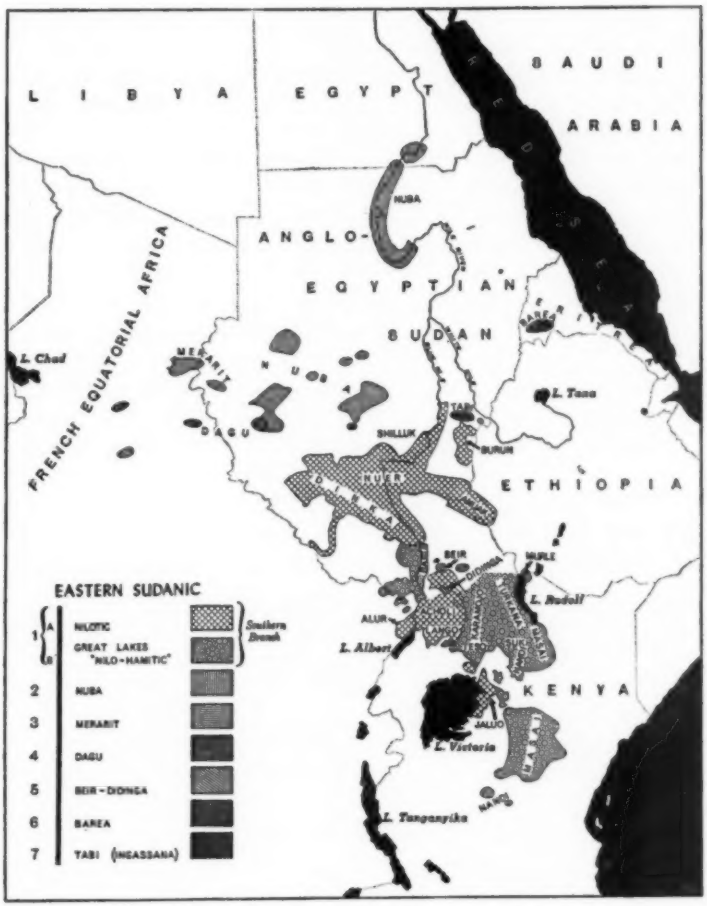

FIG. 1. Distribution of languages of the Eastern Sudanic family.

these'; (1b) Nandi *nu/ču* (<ku) 'this, these'; (3) Didinga *čyen/čyegi* 'this, these'

6. *ma relative and adjective formant.* (1a) Nilotic *ma-* (Nuer singular only); (4) Barea *-mo* (participle); (7) Dagu *ma-*

7. *ti plural demonstrative and relative.* (1a) Nuer *te;* (1b) Bari *ti* (probably Turkana *θ*, Teso *s* pronominal plurals belong here); (4) Barea *-te-* (in *-tego* adjectival plurals formed by relative *-go*)

8. *Relative and adjective formation in -ko.* (1b) Bari *ko-;* (2) Nubian *-go;* (4) Barea *-go;* (6) Merarit *-k*

9. *n feminine.* (1a) Shilluk *-n* (only in *ña-n* 'daughter'); (1b) Masai, Bari, etc.; (2) Nuba *-en* (Nile Nubian *id* 'man', *id-en* 'woman'); (5) Tabi *-ñe.* Compare also Didinga *ŋa* 'woman'

10. *-it, -et singular noun afformative.* (1a) Burun *-it;* (1b) Bari *-it, -et,* Latuka *-it;* (2) Nubian *-d, -id;* (3) Didinga *-it;* (6) Merarit *-t* (e. g. *kiŋet/kiŋe* 'tree')

11. *Plurals in -k -ka, -ki, -ku.* (1a) Burun *ku, ka,* Acholi *-gu* (relationship term) *-gi,* Shilluk *k*; (1b) Bari *ki,* Teso *-ko,* Masai *-k,* etc.; (2) Nubian *-gu*; (3) Didinga *k;* (4) Barea *-ka* (demonstrative *-gu*); (5) Tabi *-k;* (6) Merarit *ŋ* (<*k*)

12. *Plurals in -t, -ta.* (1a) Shilluk Dinka, Nuer *t*; (1b) Masai *-ta, -to,* Bari *-tă;* (2) Jebel Dair *-du;* (3) Didinga *-ta;* (4) Barea *-ta*

13. *Plurals in -n.* (1a) Nuer *-ni,* Lango *-an, -aŋ, ni,* Shilluk *-ni*; (1b) Masai, Latuka, Teso *-in,* Nandi *-en;* (2) Jebel Dair *-in;* (3) Didinga *-ni, -na, -ɛn*

14. *Plurals in -i.* (1a) Shilluk, Acholi, Lango *-i*; (1b) Masai *-i,* Bari *-e*; (2) Nubian *-i;* (3) Didinga *-i*

15. *Special plurals involving consonantal alternations.* See word list under 'cow', 'small', 'tree'

16. *Nominative singular in -i.* (2) Old Nubian *-i;* (3) Didinga *-i*

17. *Genitive singular in -o, -u.* (1b) Nuer *-a;* (2) Old Nubian *-u;* (3) Didinga *-o*

18. *Locative singular -t.* (1a) Dinka -t; (1b) Masai *t-;* (2) Nubian *-do;* (3) Didinga *-to, -ti;* (4) Barea *-t, -ta, -ti;* (5) Tabi *-te;* (6) Merarit *-ta;* (7) Dagu *-ti*

19. *Locative singular in l.* (2) Nubian *-la;* (4) Barea *-li;* (5) Tabi *-ul*

20. *Accusative singular in k.* (2) Mahas *-ka,* Kenuz, Dongola *-ki;* (6) Merarit *ŋ* (<k)

21. *Locative plural in -ni.* (1a) Nuer *-ni, -na;* (3) Didinga *-ni*

22. *Copula and tense formative a.* (1a) Shilluk, Dinka, Nuer *a;* (1b) Bari *a;* (2) Nubian *a* (copula only)

23. *Verb plural in k.* (3) Didinga *-k;* (4) Barea *-k, -ge;* (6) Merarit *-key;* (7) Dagu *-ka* (first person plural only)

24. *Future in ba, bi.* (1a) Nuer, Dinka *bi;* (1b) Nandi *i-p* (<*i-b*); (2) Nubian *fa, fi, bi*

25. *Verbal negative in m.* (1b) Masai, Nandi *m;* (2) Nubian *m-* (to be inferred from such forms as *esk* 'to be able', *m-esk* 'to be unable'); (3) Didinga *ma* (with imperative); (4) Barea *ma* (with imperative); (6) Merarit *m-*. Compare also Shilluk *ba,* Dagu *ba*

26. *Inchoative in -an.* (1a) Shilluk (nasal substitution); (1b) Bari, Latuka *-an;* (2) Nubian *-an -aŋ;* (4) Barea *-en*

27. *Verbal dative* 'to do for someone'. (1b) Teso, Bari, Masai, *-kin,* Latuka *-k,* Nandi *-ci, -cin;* (3) Didinga *-eki*

Eastern Sudanic Word List

back: (1a) South Burun *ŋeiyo:,* Nuer *ŋɛ:k,* Lango *ŋeč;* (5) Tabi *oiñ;* (6) Merarit *ŋei;* (7) Dagu *o:ŋei*

bone: (1b) Karamojo *ak-oit,* Nandi *k-o:wet,* Masai *ol-oito;* (2) Kenuz *k-i:d,* Dair *k-oi;* (4) Barea *k-etti*

backbone: (1a) Lango *oguru;* (1b) Suk *kurot,* Nandi *rot,* Turkana *egur;* (3) Didinga *karot*

to be: (1a) Nuer, Dinka *a;* (1b) Bari, Masai *a;* (2) Nuba *a;* (3) Didinga *a*

to be in a place: (1a) Nuer *te,* Anuak *ti,* Lango *tye;* (1b) Masai *tii;* (3) Didinga *-ti* (in); (4) Barea *-t, -ta, -ti* (in, upon); (5) Tabi *-te* (inside); (7) Dagu *te* (in)

belly: (1b) Turkana *etau* (heart), Lango *etau* (heart of an animal); (2) Old Nubian *tu,* Nile Nubian *tu:,* Dair *to;* (4) Barea *taua*

black: (1a) Nuer, Dinka *čol,* Lango *sol;* (3) Didinga *hol* (?); (4) Barea *sur-ko*

to boil, be cooked: (1a) Shilluk *wa:lo;* (1b) Bari *walala;* (2) Mahas *wal*

breast: (1b) Nandi *kinet,* Masai *ol-kina,* Bari *kinat;* (4) Barea *kena*

child: (1a) Nuer *ga:θ* (*gar-* in compounds); (1b) Masai *eŋ-gerai,* Bari *ŋgõrõ,* Latuka *ŋgairi-ok* (plural); (5) Tabi *aǰa:r*

cow: (1a) Shilluk *dyaŋ/dok;* (1b) Bari *ki-teŋ,* Latuka *nɛɪtɛŋ,* Teso *aki-tɛŋ,* Masai *eŋ-gi-teŋ,* Nandi *tany/tič;* (2) Kenuz, Dongola *ti:;* (3) Didinga *taŋa/tena;* (5) Tabi *tom/tok;* (6) Merarit *te:;* (7) Dagu *teinyei/tukkei*

to desire: (1a) Dinka *niar;* (1b) Bari *nyar,* Masai *nyor;* (2) Kenuz, Dongola *ner*

to die: (1a) Dinka, Shilluk, Anuak *tou;* (1b) Bari *tuan,* Masai *tua;* (2) Nile Nubian *di:,* Midob *tiwa* (present tense), Dair, Dilling, Dulman *ti;* (4) Barea *di*

dog: (4) Barea *wos;* (6) Merarit *wuši;* (7) Dagu *isi*

donkey: (1a) Dinka *akaja;* (1b) Bari *kayne;* (2) Nile Nubian *kaǵ;* (8) Dagu *kačei, kačinei*

dove: (1a) Nuer *kur,* Shilluk *akur;* (1b) Bari *gure,* Turkana *akuri;* (2) Kenuz, Dongola *kuru;* (6) Merarit *okur*

to drink: (1a) Nuer *ma:*t, Shilluk *ma:*do; (1b) Latuka *mata,* Bari *mata* (passive); (5) Tabi *mada*

enclosure: (1b) Bari *aŋ,* Masai *aŋ* (kraal); (2) Mahas *aŋgi*

to expel: (1b) Bari *tore,* Masai *itur;* (2) Nile Nubian *tur*

fire: (1a) Dinka, Nuer, Shilluk *mač;* (1b) Latuka *nɛ-ɛma,* Bari *ki-maŋ,* Masai *eŋ-gi-ma,* Nandi *maa;* (2) Mahas, Old Nubian *maša* (sun), Kenuz, Dongola *masi-l* (sun); (7) Dagu *masi*

five: (1a) Dinka *dyeč,* Nuer *ðieeč,* Southern Burun *doi, doik,* Dinka de-, do- (five—in higher numerals); (2) Kenuz *diju,* Dongola *diji,* Dair *dišu;* (4) Barea *de, ja-* (five—in higher numerals); (7) Dagu *-da, -dak* (apparently five in higher numerals)

forest: (1a) Acholi *tim,* Dinka *tim* (tree); (4) Barea *tūm* (tree)

go out: (1a) Shilluk *welo,* Dinka *fal* (leave); (1b) Bari *wala;* (2) Mahas *fal,* Old Nubian *pal,* Dair *bal;* (5) Tabi *pala* (descend)

guest: (1a) Dinka *kaman,* Shilluk *kemo* (to visit); (1b) Bari *komo:nit;* (4) Barea *hömet/hömena*

hand: (2) Mahas *eddi;* (3) Didinga *aðit/aðina;* (4) Barea *ad*

to hate: (1a) Shilluk *ma:no,* Dinka *man;* (1b) Bari *man,* Masai *men* (despise); (2) Kenuz, Dongola *mo:n*

head: (1b) Bari *uret* (crest); (2) Dongola, Kenuz, Mahas, Gulfan *ur;* (5) Tabi *ol;* (6) Merarit *ure*

hippopotamus: (1a) Shilluk *rau,* Dinka *rou,* Nuer *rɔu;* (1b) Bari *yaro;* (2) Kenuz, Dongola *erit*

house: (1a) Anuak *ka* (place), Lango *ka* (place); (1b) Nandi *ka;* (2) Nile Nubian *ka:*

jaw: (1a) Dinka *gem* (cheek), Nuer *jyom* (cheek); (1b) Bari *nyekem;* (2) Kenuz, Dongola *jakum*

male: (1a) Dinka *moč;* (3) Didinga *mač;* (6) Merarit *mo*

man: (2) Old Nubian *it*, Kenuz, Dongola *id;* (3) Didinga *ɛt/ɛta;* (4) Barea *eite*

meat: (1a) Anuak, Shilluk, Lango, Dinka *riŋo*, Nuer *riŋ;* (1b) Turkana *aki-riŋ*, Karamojo *agi-riŋ*, Masai *eŋ-gi-ri;* (3) Didinga *idiŋ*

milk: (1a) Dinka *ča*, Nuer *čak*, Shilluk *čak;* (1b) Nandi *če;* (2) Mahas *su*, Birgid *eši*, Kenuz, Dongola *ičі;* (4) Barea *sa/sata;* (6) Merarit *zuŋ* (probably <zuk)

mother (my): (1b) Suk *iyu*, Nandi *eiyo;* (2) Kenuz, Dair, Midob *iyo*

mouth: (2) Mahas *ag*, Kenuz, Dongola *agil*, Koldegi *aul;* (4) Barea *aulo;* (5) Tabi *ag;* (6) Merarit *kul;* (7) Darfur Dagu *akkei*

name: (1a) Dinka *rin;* (1b) Bari *k-arin;* (2) Dongola, Kenuz *eri;* (4) Barea *ade*

neck: (1a) Shilluk *muto;* (1b) Nandi *ki-mut*, Bari *murut*, Masai *en-murtu;* (2) Nile Nubian *gumur*

night: (1a) Shilluk *war*, Nuer *war*, Lango *owor;* (1b) Karamojo *awara, akwar*, Masai *eŋ-awarie;* (2) Mahas *awar*, Dulman *kwalo* (at night)

person: (1a) Dinka *koi;* (2) Nile Nubian *ko:;* (4) Barea *ku*

to pour: (1a) Dinka *byok*, Nuer *bok* (throw); (1b) Bari *buk*, Nandi *apuk*, Masai *i-buɣ;* (2) Kenuz, Dongola *bog*

rain, water: (1a) Dinka *uar* (river); (1b) Masai *eŋ-are* (water), Latuka *na-are* (water), Karamojo *agi-ru* (rain), Turkana *aki-ru* (rain), Bari *k-are* (river), *ru* (to water); (2) Kenuz, Dongola *aru* (rain), Old Nubian *aru* (to water), Dair, Gulfan *are* (rain); (6) Merarit *ar* (sky, rain); (7) Darfur Dagu *k-o:rei* (rain)

to seize: (1a) Dinka *dɔm;* (1b) Bari *dum*, Masai *tum;* (2) Mahas *dumm*

sheep: (1b) Masai *eŋ-g-err*, Latuka *na-k-er;* (2) Dair *or-ti*, Mahas *ur-ti;* (5) Tabi *er/erik*

shield: (1a) Shilluk *kud*, Dinka *kot, kud;* (2) Gulfan *koru*, Dair *kori*, Dongola, Kenuz *karu;* (4) Barea *kodo/kotta*

small: (1a) Shilluk *ten*, Dinka *tine, -tet* (diminutive suffix), Nuer *tɔ:t/tɔanə, tot* (child); (1b) Masai *dito* (child), Bari *dit;* (2) Nile Nubian *to:(d)/tu:ñi* (child), *-to:d* (diminutive); (4) Barea *di/dad* (child), *didi* (small); (6) Merarit *tadan, tatt* (child)

smoke: (1b) Nandi *puret*, Latuka *aburo*, Karamojo *apuru;* (3) Didinga *burɛt*

snake: (1a) North Burun *kak*, South Burun *kaŋ;* (1b) Latuka *kak;* (8) Dagu *kokei*

some: (1a) Dinka *ketoi* (a trifle); (1b) Bari *čit*, Nandi *kitegin* (a little of something), Masai *kete* (little); (2) Mahas *kid;* (3) Didinga *kidiča/kidičik*

spear: (1a) Nuer *muθ;* (5) Tabi *mus/musak*

to split: (1a) Dinka *kak* (cut), Shilluk *ka:go,* Lango *kako,* Nuer *kage;* (1b) Bari *kagu;* (2) Nile Nubian *kak*

star: (2) Dair *winji,* Kadero *u:n-du;* (4) Barea *wini*

to steal: (1a) Shilluk *kwalo,* Dinka *kwal;* (1b) Bari *kola;* (4) Barea *hol*

stick: (1b) Karamojo *agulit;* (2) Nile Nubian *ga:le;* (5) Tabi *ma:gult*

stone (No. 1): (1a) Anuak *kidi,* Shilluk *kit,* Lango, Alur *kidi;* (2) Nubian *kid;* (6) Erenga Merarit *kat*

stone (No. 2): (1a) Dinka *kur, god,* Lango *got,* North Burun *gur-it;* (1b) Bari *kodu;* (2) Nile Nubian *kul,* Birgid *kur;* (6) Merarit *kura*

sun: (1a) Dinka *ak-ɔl;* (1b) Bari *k-oloŋ,* Masai *eŋ-g-oloŋ,* Karamojo *ak-oloŋ,* Turkana *ek-oloŋ;* (3) Didinga *k-or;* (7) Dagu *uroŋei*

to be sweet: (1a) Dinka *mit,* Shilluk *mɛt;* (3) Didinga *med*

ten: (1b) Bari *mere;* (2) Dair, Dulman, Koldegi, etc., *bure;* (6) Merarit *mer*

thing: (1a) Acholi *gi/gik,* Shilluk *gin/gik;* (1b) Nandi *kii;* (3) Didinga *gi*

throat: (1b) Masi *ol-gos;* (2) Nile Nubian *go:s;* (4) Barea *koso*

tooth: (2) Mahas *nid,* Kenuzi *nihta, ni:ta;* (3) Didinga *nigitat/nigit;* (4) Barea *nihi/nihitta;* (5) Tabi *niet, nyigitu*

tree: (1a) Shilluk *yat/yɛn,* Nuer *jiat/jɛn;* (1b) Latuka *nä-yäni,* Masai *ɔl-ǰani,* Nandi *kɛt;* (2) Birgid *ka:n;* (3) Didinga *kɛt/kɛna;* (6) Merarit *kidi/kiŋ*

two: (1a) Shilluk *aryau,* Dinka *rou,* Nuer *rɛu;* (1) Latuka *arega,* Masai *are,* Bari *öri;* (2) Dair, Gulfan, Koldagi *ora,* Dulman *ore;* (4) Barea *arega;* (6) Merarit *wari*

urine: (1a) Anuak *la;* (1b) Masai *eŋ-go-lak,* Bari *ko-la;* (3) Didinga *xu-la*

water: (1a) Nuer *pi/pik,* Acholi, Lango, Shilluk, Luo *pi,* Burum *piuk;* (1b) Teso *aki-pi,* Nandi *peik,* Karamojo *agi-pi;* (5) Tabi *fik, feko*

white: (2) Kenuz, Dongola *aro:*; (3) Didinga *ora*; (4) Barea *er-enko*

who? (1a) Nuer, Dinka, Anuak *ŋa;* (1b) Nandi *ŋo,* Karamojo *ŋai,* Bari *ŋa,* Latuka *ŋai;* (2) Mahas *na:,* Kenuz, Dongola *nai;* (3) Didinga *ŋani;* (4) Barea *na, nan, nani;* (6) Merarit *na*

wing: (1a) Shilluk *ober;* (2) Mahas *awir,* Kenuz, Dongola *aur, abir,* Old Nubian *aur;* (1b) Masai *opir, obir,* Bari *k-upir;* (3) Didinga *wir* (to fly)

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PLANT EVIDENCE FOR EARLY CONTACTS WITH AMERICA

GEORGE F. CARTER

INTRODUCTION

THE QUESTION of direct contacts between American centers of high culture and those of the Old World has been a vexing problem. Extremists on one side have claimed Egyptian influence in America and extremists on the opposite side have denied any contact with America by anyone at any time. We need to review the evidence. In such a review, the plant evidence suggesting contacts with Old World agricultural peoples offers certain unique advantages. Plants are not constructs of the mind. They are less subject than most data to appeals to the psychic unity of mankind, parallelisms, independent inventions, limitations of techniques, and so forth.

But even plants do not supply us with absolute answers. The possible transfer of plants across oceans by some most unusual means; the remote chance that man on both sides of the ocean might then domesticate the plant or use it for identical purposes, and even give the plant a similar name, can not be dismissed. The possibility always exists. On the other hand we can show that such things *probably* did not happen. The whole evolution of the plant world indicates that the oceans have served as highly efficient barriers to plant movement. The history of plant domestication, while it suggests plural origins of agriculture, shows that it has been quite rare for man to domesticate the same or closely related plants in areas not in contact with each other.

When we deal with more than one plant which can be shown probably to have been transferred by man across the ocean, the "probables" have a cumulative effect. The cumulative nature of the evidence is important. Neither the individual plants, nor the plant evidence as a whole, stands alone. Rather, the plant evidence is part of a complex whole, and the coherency of the whole can legitimately be used in defense of the validity of the parts.

Some domestic plants reproduce vegetatively only and only through the intervention of man can they cross such water barriers as the oceans. We have in them unique markers. As O. F. Cook remarked nearly fifty years ago,

> the same plant does not originate twice, and [that] varieties dependent everywhere for their very existence on human care must also have been distributed by human agency.[1]

1 O. F. Cook, *Food Plants of Ancient America* (Annual Report, Smithsonian Institution, 1893, pp. 481-497), p. 484.

[And again:] For the present purposes it suffices to remember that the actual introduction of plants by human agency discounts in advance all objections on the ground of distances and difficulties of communication, and justifies the fullest use of biological or other data in tracing the origin and dissemination of agricultural civilization in the Tropics of both hemispheres.[2]

THE EVIDENCE OF CULTIVATED PLANTS

The sweet potato:

The sweet potato (*Ipomea batatas*) is an American plant. Yet before 1864 it was noted that in Polynesia it was known to the natives by the Quechua name of *kumar*.[3] In 1888 Hillebrand, writing with the background of long residence in the Hawaiian Islands and acquainted with their legends and history, wrote:

> The Sweet Potato, of aboriginal cultivation, was, next to the taro, the principal article of food to the natives at the time of discovery. They enumerate about twenty varieties which differ as much in shape, color, and quality of the tuber as in the shape of the leaves. The vernacular name "Uala" corresponds well with the Tahitian "Umara" and the New Zealand "Kumara," and it is interesting to note that B. Seeman has met with the same verbal root "Cumar" for this esculent among the Quichuas of Ecuador.[4]

These data, and they could be multiplied, seem not to have impressed Americanists.

In 1932 Dixon, who was no protagonist for trans-Pacific influences,[5] reported on the question of the sweet potato in the Pacific.[6] From an examination of the historical accounts of the voyages of discovery he concluded that the sweet potato was pre-Spanish in both Polynesia and Melanesia. Dixon noted that the Maori legends state that the sweet potato had been carried to New Zealand both by the migrants of the fourteenth century and those of an earlier migration. The retention of the name for the sweet potato by the Chatham Islanders suggested to Dixon the likelihood of the validity of the earlier introduction.

That the plant had been in Polynesia for a considerable length of time is

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2 *Idem*, p. 496.

3 Asa Gray and J. H. Trumbull, *Review of De Candolle's Origin of Cultivated Plants with Annotations upon certain American Species* (American Journal of Science, series 3, vol. 25, 1883), pp. 241-255, 370-379. On p. 248 they call attention to Markham's noting in 1864 Seemann's recording the identity of name for sweet potatoes in Polynesia and Peru.

4 W. F. Hillebrand, *Flora of the Hawaiian Islands* (Heidelberg, 1888), p. 314.

5 R. B. Dixon, "Contacts with America across the Southern Pacific" (in *The American Aborigines*, Diamond Jenness, ed., Toronto, 1933).

6 *Idem*, *The Problem of the Sweet Potato in Polynesia* (American Anthropologist, vol. 34, pp. 40-66, 1932).

indicated by the linguistic changes that the name *kumar* underwent in the various islands. Since the sweet potato is vegetatively reproduced, variation would expectably be quite slow. Yet Hawaii had many varieties and this, also, must mean either considerable antiquity or importation of a considerable range of varieties. In Hawaii also Dixon noted that the plant is referred to in the most archaic cosmogonic chants and myths and is associated with the major gods. It has elaborate rituals associated with its planting, cultivation, and storage. Evidence from traditions, botany, language, and ritual all suggest antiquity for the crop.

There is also physical evidence. At the time of discovery New Zealand had extensive areas of land in sweet potatoes. Pits from which gravel was dug for hilling the potatoes covered many acres and some had been excavated to depths of five or six feet. In some of these pits trees of great age were growing. Excavation of an acre of ground to a depth of five feet involves the removal of approximately twenty million pounds of gravel.[7] The loads may have been basket loads of perhaps five pounds each. That suggests four million loads of gravel carried for each acre of ground so excavated. When it is remembered that there were many acres, and that the work would have been done intermittently, then an impression of a considerable elapse of time is gained.

Dixon failed to find any account of contacts with America in the Polynesian traditions. He thought that had such a contact been made in the period of long voyages between the tenth and fourteenth centuries, the account would be preserved. He suggested, therefore, that the contact may have been made in the initial period of voyaging, centuries before 1000 AD. This would be in agreement with his suggestion that the importance of the plant in marginal, and its unimportance in central, Polynesia[8] may be related to diffusion prior to the influences on central Polynesia in medieval times.

The several lines of arguments are mutually supporting and indicate an importation from the area where *kumara* was the name for the sweet potato at a period long preceding 1000 AD. There can be no question that man carried the plant. The fact of vegetative reproduction and the Quechua name establish proof beyond all reasonable doubt. Only the absolute date remains in doubt.

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7 Figuring 43,650 sq. ft. per acre, times 5 feet deep, times 100 lbs. (wt. per cub. ft. of gravel) equals 21,825,000 lbs.

8 The unimportance, if not complete absence of the sweet potato in central Polynesia, is an interesting though obscure point. As Dixon noted, the retention of sweet potatoes in Hawaii and Easter Island can not be related to absence of other foods. Hawaii especially was as well supplied with food crops as central Polynesia. Abandonment of useful items seems to have been a peculiarly Polynesian trait. One wonders what the explanation is.

Hibiscus tiliaceous:

It would be strange if there were contacts between the New World and Polynesians of such intimacy that they led to the exchange of a plant together with its name, if the contacts were limited to one plant. Cook long ago pointed out that this was not the case.[9] He noted that *Hibiscus tiliaceous* grew wild in America and was used in the same way as in Polynesia. Oviedo, Dampier, Sloane, Barrerre were cited by Cook as describing the plant and its uses. Cook found the plant to be widely distributed in tropical America and to be known under the name of *maho* or *mahagua,* or some variant of this name. In Polynesia he noted the plant is also widespread, wild, widely used and known as *mao, mau, au, hau, fau, vau; moanua,* the closest approach to *mahagua,* being found on Easter Island. In America he found the name also to be applied locally to many other fiber-bearing plants such as *Thespesia, Ficus, Agave,* and so forth. Cook was wedded to the idea that agriculture began in America and spread thence across the Pacific to Asia. Quite naturally, therefore, he concluded that man had carried the *Hibiscus,* also. The identity of names and usages in parallel to the sweet potato were convincing to him, and, one would think, to anyone.

However, the mental climate was then even less receptive to ideas of early trans-Pacific contacts with America than it is now. A challenge to Cook's ideas was bound to be made. Merrill took up the cudgels almost at once.[10] He noted that the plant had a pan-tropic strand distribution; that it was never cultivated in the tropics of the Old World or New World outside Polynesia because better fiber sources existed in those areas. Its seeds are perfectly adapted to float for months, hence its presence on two sides of an ocean can not be evidence that man carried it. He argued that the fact that the word for this hibiscus in Polynesia was applied to many bast fiber plants and could be traced back into Malaya showed that the word was a general one for bast in Indo-Malaya and only had its specific meaning of hibiscus in Polynesia. He concluded therefore:

> The probabilities are very great that all of the Polynesian *mao* series are merely modifications of the Indo-Malayan *bago* series; and that the Polynesians in their migration, having adopted the name while in the Indo-Malayan region, merely applied it to the wild plant which they found all over Polynesia. It would seem, therefore, that this root has nothing to do with the tropical American *maho* series, the resemblances being merely accidental.

9 O. F. Cook and R. C. Cook, *The Maho or Mahagua, as a Trans-Pacific Plant* (Journal, Washington Academy of Sciences, vol. 8, pp. 153-170, 1918).

10 E. D. Merrill, *Comments on Cook's Theory as to the American Origin and Prehistoric Polynesian Distribution of Certain Economic Plants, especially* Hibiscus tiliaceous *Linn.* (Philippine Journal of Science, vol. 17, pp. 377-384, 1920).

These arguments seem to me to be excellent specimens of the result of fixed ideas. Cook was so intent on proving the American origin of agriculture that he was incautious, if not unwise, in using a halophytic plant with a seed well adapted to water transportation as *proof* of man's carrying plants across the ocean. Merrill on the other hand was either so incensed by Cook's special pleading or so allergic to trans-Pacific contacts (or both) that the violence of his reaction blinded him to the virtues of Cook's arguments.

Cook pointed to a plant common to America and Polynesia, used in identical ways, and known by names of near identity. He made two further important points. There was an exact parallel to this plant in the case of the sweet potato; and the plant name in America was applied not only to hibiscus but to other fiber bearing plants. Merrill pointed to seed qualities as invalidating the argument for the *necessity* of man carrying the plant, but did not realize that this did not amount to showing that man did *not* carry the plant. Winds and currents suggest that if the plant was carried across the Pacific by natural means it must have been from America to Polynesia. But natural carriage would leave the problem of usages and name to be solved. To argue, as Merrill did, that the name in Polynesia was derivable from a generic term for bast while ignoring evidence suggesting the same thing in America is weak indeed. To ignore the parallel of hibiscus to the sweet potato is only understandable from a history of ideas standpoint. The identity of names and uses in Polynesia and America, when coupled with the positive evidence from the sweet potato, makes it certain that whether or not the plant crossed the seas by natural means, man carried the name for the plant and quite possibly the usages across the same seas. It even seems probable that he carried the plant also.

The yam:

The yam (*Dioscorea alata*) presents an equally interesting problem. It probably was domesticated in Java and spread from that center into Polynesia, perhaps with the original migrants. The yam is a plant widespread in Melanesia and Polynesia and is mentioned in China in the second or third century AD according to Laufer.[11] Yet it seems to have been present in the Caribbean when Columbus reached there. This evidence has been canvassed by competent scholars.

Asa Gray and J. Hammond Trumbull reviewed De Condolle's *Origin of Cultivated Plants* and dealt at length with some of the plants described by the early chroniclers of the Caribbean.[12] They found that three root crops—sweet potatoes,

11 Berthold Laufer, *The American Plant Migration* (Scientific Monthly, vol. 28, pp. 239-251, 1929), p. 245.

12 *Op. cit.*, pp. 250-253.

yams, and manioc—were described from the time of earliest contact with the Caribbean. Although there was occasional confusion between all three, the identity of each is clearly established. Oviedo's account of 1535 contains a passage describing the shape, venation, stem, and the hanging habit of the leaf of the yam, differentiating this plant from the sweet potato, and giving the native name as *ajes*. *Ajes* are described in Navarette's account of Columbus' voyage. Gray and Trumbull noted that Alexander von Humboldt had reached the same conclusion, as early as 1811.

Here then is another plant which, like the sweet potato, is propagated vegetatively and hence most unlikely to cross wide seas by wind, drift, birds, or other non-human agencies, but which crossed the ocean in pre-Columbian times. The Caribbean location might suggest an African source for this plant, though there is little evidence that the yam was known in Africa at this time. The recording of the yam in the Caribbean, rather than on the Pacific coast of Central America may be but an accident of history. At the moment we have no insight into the time when the yam was carried into America. It could have been brought by those who carried the sweet potato back into the Pacific.

The bottle gourd (Lagenaria siceraria):

The bottle gourd is probably a native of India according to De Candolle, who also notes Sanskrit names for it and quote Bretschneider as his authority for its fifth or sixth century appearance in China.[13] The bottle gourd is, however, archaeologically well documented in America. In Peru it has long been known to be at least as early as Nasca.[14]

Recent work has produced a pre-Chavin agriculture containing the bottle gourd but lacking corn.[15] Guesses at the age of this material suggest something earlier than 1000 BC and possibly as old as 3000 BC. These are guesses, but highly interesting ones in view of the probable Asiatic origin of the bottle gourd and the implied arrival of Old World agricultural peoples in Peru at this early date. If the bottle gourd was brought from Polynesia, where it is a very important plant, this date would be in interesting agreement with Dixon's suggestion that the sweet potato evidence suggested that contact with America may have been

13 A. De Candolle, *Origin of Cultivated Plants* (London, 1884), pp. 245-249.

14 Cf. E. Yacovleff and F. L. Herrera, *El mundo vegetal de los antiguos peruanos* (Revista del Museo Nacional, Lima, Peru, vol. 3, pp. 243-322, 1934; vol. 4, pp. 31-102, 1935), p. 303; L. M. O'Neale and T. W. Whitaker, *Embroideries of the Early Nasca Period and the Crop Plants Depicted on Them* (Southwestern Journal of Anthropology, vol. 3, pp. 294-321, 1947).

15 Junius Bird, *America's Oldest Farmers* (Natural History, vol. 57, pp. 296-303, 1948). Since this was written dates based on Carbon 14 have established the age of this material as prior to 1000 BC.

made long prior to 1000 AD, though Dixon was not thinking of 1000 BC or earlier.

It is inevitable that some one should appeal to winds and currents to bring the bottle gourd to America. Its shell is thick and hard. Perhaps then it could float here. Botanists have long discussed the possibilities of plants being drifted across the seas. Few plant seeds or fruits are given any chance of surviving such lengthy soaking and just as little chance of successfully competing on a foreign shore. Those for which there is such evidence are shore dwelling plants adapted to saline conditions, possessed of highly resistant seeds, and able to root and flourish in strand locations.[16] *Lagenaria* possesses none of these qualities and the occurrence of the same genus and species in both the Old World and the New is probably man's work. We need to know more about the history of *Lagenaria*. We need particularly to inquire into its African distribution. The evidence already at hand, however, is in agreement with that presented for sweet potatoes and yams. If man carried them across the sea, then it seems probable that he also carried the bottle gourd. It remains possible that the gourd floated across and was independently seized upon by man in America. But that is a remote possibility as opposed to the very probable human carriage. Hence I would conclude that *Lagenaria* probably was carried by man across the oceans, probably across the Pacific, at a time well before 1000 AD, and if the Peruvian guess-dates are valid, well before 1000 BC.

Cotton (*Gossypium* sp.):

Cotton has recently had extensive genetic study.[17] It has been found that the cottons of the world can be placed in three groups: Old World with 13 large chromosomes, whether wild or domesticated; American wild with 13 small chromosomes; and American cultivated and Hawaiian wild with 26 chromosomes, of which 13 are large and 13 are small.

Genetic study shows that the 26-chromosomed cottons contain both the Asiatic and American set of chromosomes. This indicates that at some time after the differentiation of the cotton genera the Asiatic and American cottons have been grown alongside each other and a crossing accompanied by a doubling of chromosome numbers has occurred. One must assume a common center of origin for all

16 For an excellent discussion of the probability of plants crossing oceans see E. V. Wulf, *An Introduction to Historical Plant Geography* (Chronica Botanica, Waltham, Mass., 1943). See especially Chapter 8, "Natural Factors in the Geographical Distribution of Plants."

17 This discussion of cotton rests in large part on the two slim but important papers here cited: J. B. Hutchinson, R. A. Silow, and S. G. Stephens, *The Evolution of Gossypium and the Differentiation of the Cultivated Cottons* (Oxford, 1947); S. G. Stephens, "Cytogenetics of Gossypium and the Problem of the Origin of New World Cottons" (in *Advances in Genetics*, vol. 1, pp. 431-442, New York, 1947).

cottons. Their separation for a lengthy period of time is necessary to allow the development of the Old World and the New World genom types. Thereafter, the American and Asiatic types must be brought into proximity to each other in order that the 26-chromosomed American domestic type (and Hawaiian wild) be created.

Originally the crossing of the two 13-chromosomed species was thought to have occurred in the geologic past. Appeals were made to land bridges across the Pacific as a means of bringing these separate species together. More recently numerous difficulties with this argument have been pointed out, not the least of these being that not one but two linkings of the Old World and New World land masses would be called for and that there is no geological support for any such trans-Pacific land bridge at any time. To account for the distribution of the two sets of wild cottons (Afro-Asian and American) one must accept some form of Wegener's hypothesis of drifting continents. But not even the followers of Wegener claim the facile union-separation, union-separation of continents seemingly required to develop the present cotton picture.

Other postulates can be made to defend an ancient origin of 26-chromosomed cottons. Perhaps the Asiatic genom was anciently present in America, crossed with the American genom, and thereafter died out in America though surviving in Asia. This, however, is contrary to the outstanding feature of the taxonomy of *Gossypium*—that is the way in which diversification has accompanied geographical distribution.[18] To quote Hutchinson, Silow, and Stephens,

> Evidently the continental groups which carry these widely distinct genoms must have been isolated from each other for a very long period, and in view of the fact that they are inhabitants of arid regions, and bear seeds that are not adapted to distribution over long distances by water, it can only be supposed that they reached their present areas by dispersal across land masses with a dry climate.[19]

To accomplish this they appeal to Wegener's theory of drifting continents and point to Wulf's utilization of this theory to explain similar phenomena.

Much of the tendency to appeal to an ancient crossing of the Asiatic and American cottons and to land bridges stems from two factors. First, 26-chromosomed cottons found wild in the Pacific Islands (Fiji, Marquesas, Hawaii, Galapagos) were thought to be remnants of an ancient distribution. In addition to other evidence to be presented below, the ecological niche occupied by the cottons in these islands suggests that they are introduced plants, possibly escapes from cultivation. Secondly, it was believed that man could not have been instrumental

18 Hutchinson *et al.*, p. 53.
19 *Idem*, p. 46.

in bringing the two forms together. Sweet potatoes, yams, and bottle gourds alone —and they do not stand alone—show that man could have brought the Old World and New World cottons together in recent times, perhaps as early as 3000 BC.

Genetic study of cotton also has changed our understanding of the cottons of the Pacific Islands. All but the Hawaiian cotton have now been shown to be but derivatives of American domestic forms and not to be early or ancestral forms. Indeed, they can be shown to be derived from specialized forms of relatively late origin—late origin in a botanical sense, for Captain Cook collected cotton in Polynesia. That on the Galapagos islands is a little differentiated form of *Gossypium barbadense*, the South American cotton. Those in Fiji and Marquesas are *G. hirsutum punctatum* and are similar to a Caribbean variety of *punctatum*. This indicates that the hybrid cottons of America had already differentiated before they were carried back into the Pacific. The genetics of cotton suggest that the *barbadense* cottons are the basic type from which *hirsutum* can be derived, just as *punctatum* must in turn be derived from *hirsutum*. This is not, however, a reversible sequence. *Punctatum* in Polynesia is a late development, not an early, basic, or primitive form.

This has important cultural meaning also. It suggests an early introduction of Asiatic cotton into America and the creation by hybridization of new types of cotton in South America. After these new cottons had diffused to Mexico and after the passage of sufficient centuries to allow the differentiation of new species and beyond that of a variety of that new species, this variety was carried back into the Pacific at least as far as Fiji. This was accomplished so early that the plant was later abandoned and apparently all memory of its introduction and use lost. This suggests, then, an early voyage by Asiatics to America, and many centuries later another voyage from America to Polynesia.

Still another pertinent bit of information is that of all the American wild cottons only *Gossypium raimondi* of Peru seems likely, when combined with Asiatic cotton, to give rise to cotton of the American 26-chromosomed types. It is probably no accident that the earliest cotton growing known in the New World is in Peru, that the bottle gourd—an Old World plant—is found right with it, and that when the sweet potato was carried from America into Polynesia, it was carried from the same area.

Even the endemic cotton of Hawaii is suspect. Its 26 chromosomes and its possession of spinnable lint suggest American origin and a history as a fiber plant. The presence of sweet potatoes shows that plants of American origin were carried that far. The bigger mystery concerns why it was carried. Was cotton origin-

ally carried as a source of oil seeds, as suggested by Hutchinson, Silow, and Stephens?[20] Or was weaving later given up in the Pacific area for bark cloth? Is part of the answer to the latter to be found in Bird's finding that the weaving in his early Peruvian site was done without the use of the heddle?[21] Would bark cloth be able to replace such primitive weaving, though it would probably not replace true loom weaving? Or, was cotton weaving little understood and unable to replace an established bark cloth industry in Polynesia?

There is probably no such thing as absolute proof of anything. Land bridges can not account for the plant picture. Man did cross the Pacific bearing domestic plants. Genetics suggest cotton was carried by man to America. That man carried Asiatic domestic cotton to the New World at an early date is the simplest reasonable explanation. Other explanations are more complicated and improbable. I am inclined, therefore, to accept cotton as another of the plants carried across the Pacific and use it to explore the possible time that man may have accomplished this crossing.

Cotton appears with the bottle gourd (which is clearly Asiatic in origin) in Peru at the site previously mentioned, which is guess-dated between 1000 BC and 3000 BC. These guess-dates can be checked in two ways.

An original domestication of cotton for its oily seeds in southern Arabia or northeastern Africa has been suggested. From there it was carried into the Sind. By the time of Mohenjo Daro (1500 BC) cotton of modern Indian type was being expertly woven, suggesting that linted cottons had been developed well before this date. It seems likely that by this time the plant was being diffused eastward toward Bengal and Java and beginning its differentiation into *Gossypium arboreum,* the type which would have been available to man sailing from this area out into the Pacific. One must also envision the possibility of direct sailing from some part of India. Here we stop on the Asiatic side. By 1500 BC linted cottons existed for diffusion eastward. How long prior to 1500 BC, how long they took to diffuse eastward, and when they may have been carried to Peru, we do not know.

On the American side we have absolute dating only in the southwestern United States. Cotton appears here by 700 AD. It has diffused nearly three thousand miles through varied climates and cultures. Only *Gossypium hirsutum punctatum* is known in the Southwest. This is a species differentiation from the original American cotton *Gossypium barbadense* and a subspecies differentiation beyond that. This speciation *may* have taken only a number of centuries, but it

20 *Op. cit.,* p. 70.
21 *Op. cit.,* p. 334.

could as well have taken several millenia. Should we be ultra conservative and allow only three hundred years for the differentiation of *punctatum* from *hirsutum,* only seven hundred years for the differentiation of *hirsutum* from *barbadense,* and consider diffusion to have been instantaneous, we would have a date for the origin of *Gossypium barbadense* in Peru at 300 BC. Prior to that date, if Hutchinson, Silow, and Stephens are right, Asiatic cotton must have been introduced into Peru and grown for some indeterminate period in the proximity of the wild cotton of Peru (*Gossypium raimondi*). An unusual crossing of these plants gave rise to new types of cotton which were seized upon by man and formed the basis for *Gossypium barbadense.*

The assumptions of instantaneous diffusion and improbably short periods of differentiation of species makes such a date as 300 BC rather meaningless. If we compare the rate of diffusion of traits from the Near East to Europe in early times, we find that useful traits often took a thousand years to go a lesser distance through fewer climates and cultures. Botanists seem to think of speciation as a matter of thousands if not millions of years. Bird's guess-date of 3000 BC is then not at all impossible. Neither, of course, is it proven.[22]

MAN AND COSMOPOLITAN WEEDS

Some plants have a nearly world wide distribution. They appear on both sides of the Pacific as soon as botanical records begin. They have been labelled as cosmopolitan weeds and generally attributed to accidental distribution by man in the post-Columbian period. Some of them are of interest to this problem of trans-Pacific movements.

Argemone mexicana, the Mexican poppy, is a good example. All of the *Argemones* are American. The homeland of *mexicana* is Central America and the West Indies. From there is has spread through the warm tropics of the world. A glance at Fedde's monograph on the *Papaveracea,* where the uses of this family of plants get a cursory examination, shows that *Argemone mexicana* is used medicinally in many areas.[23] Is it not strange that a mere weed accidentally carried should come to enter so many primitive pharmacopoeias? One finds in Fedde's very brief list

22 Since this was written the dating of materials in Peru and elsewhere in America has been advanced greatly by application of the Carbon 14 method. Dr Junius Bird informs me that the mound he excavated in Peru probably was abandoned prior to 1000 BC and that cotton of the *G. barbadense* type (26 chromosomes) appears at the base of this fifty foot mound. It is obvious then that the time of origin of 26-chromosomed cottons and, by implication, the time of first contacts between Asia and Middle America, are long prior to 1000 BC.

23 Friedrich Fedde, "Papaveraceae-Hypocoideae et Papaveraceae-Papaveroideae" (in A. Engler, *Das Pflanzenreich,* vol. 4, Leipzig, 1909), p. 82.

that in Java and in North America the milky sap is used for chronic skin diseases and warts. In Africa and in the West Indies the seeds or oil from the seeds are used medicinally. These items suggest that we are dealing with no weed, but a plant with culturally determined usages. That the plant and its specific usages travelled together suggests purposeful rather than accidental transport.

The question of *Argemone alba* var. *glauca* in Hawaii then assumes a new light. In Hawaii it accompanies the sweet potato, surely an American plant, and 26-chromosomed cotton of probable American origin. It has the same arbitrary medical uses as in America: not only the use of the seed oil, but also the application of the milky sap to chronic skin diseases. The plant was present when Cook discovered the islands. Fedde long ago noted that this plant grew in open places, a characteristic of the introduced plants of the islands as Engler had even earlier noted. Fedde considered it not an ancient introduction. Such evidence certainly suggests that man carried it to Hawaii.

If such a plant was carried to Hawaii, were there more? Here are a few gleaned from Hillebrand's *Flora of Hawaii*.[24] These are stated to be related to American plants and are not designated as introduced after Cook's discovery of the islands. Hillebrand referred to these and others as comprising an Andean element in the flora of Hawaii. The notes following each plant are also from Hillebrand.

Portulaca oleracea L.: The pigweed of gardens and cultivated grounds, common on all the islands. Native name *ihi*. About sixteen species chiefly belonging to tropical America.

Hibiscus youngianus: In marshes and abandoned taro-patches here and there on all islands. Native name *akiohala*. Is nearly related to the American *H. bifurcatus* Cav.

Erigeron albidus: A native of tropical America, but now a common weed in many countries of the warmer zone.

Physalis Peruviana: A native of Brazil, but naturalized in many warm countries. Native name *poha*. Used for its edible berries.

Ipomea acetosafolia Roem. and Sch.: Native name *hunakai*. A native of the West India Islands, Guiana, and Brazil where it grows on sandy sea shores.

Amaranthas paniculatus: In general cultivation.

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24 *Op. cit.* Hillebrand was in the Hawaiian Islands from 1851 to 1871. He learned the language, travelled widely, and because of his medical training noted usages of plants. Hillebrand's arrival was, then, but seventy-three years after Cook discovered the islands. His notes on what plants were present, and his judgments of what had long been present and what recently introduced, are invaluable.

Chenopodium sandwichensis
Potamogeton pauciflorus Pursh.: A North American species.
Eragrostis mexicana: from Mexico, Venezuela and Cuba.

Portulaca oleracea was in America before Columbus, according to Gray and Trumbull.[25] It was also widespread in Polynesia. However, Dr Hugh Cutler writes to me as follows:

> There are several *Portulacas* native to the New World, edible as greens and difficult to distinguish from purely Old World forms. In addition, some of the similar plants, like *Sesuvicem portulacastram* L. (called *verdolaga de la playa*) are eaten as greens occasionally and are easily confused.[26]

What Columbus described as *verdolaga* must then remain in doubt. The possibility that he described *Portulaca oleracea* remains. The question of why the *Portulacas* of the Old World and New should be so alike is also a question worthy of consideration.

Hibiscus by its distribution suggests an introduced plant. *Hibiscus tiliaceous* was enormously useful to the Polynesians as a source of bast fiber. *Physalis* is an edible plant related to the American tomato. Jenkins[27] has recently published *The Origin of the Cultivated Tomato*. He considers *Physalis* to be an older plant food than the cultivated tomato and the tomato to have been domesticated because of its general similarity to *Physalis*. Like cotton, sweet potato, and *Hibiscus, Physalis* points again toward Peru. *Ipomoae* is, of all these plants, the only shore dweller and hence the only one at all likely to be transported by sea.

Amaranth and *Chenopodium* are both ancient cultivated plants in America. For the rest of the list I have no information of possible usages but this may be my ignorance. Even these few notes from an old botany suggests a clue to the origin of some of the American element in the flora of Hawaii. It would be strange indeed if it should prove that only the "cosmopolitan weeds" used by man in America were transported by nature to Hawaii and that the same uses found in America travelled with them as in the case for *Argemone*. Yet that this may well be the case is indicated. Fossberg,[28] in a recent study of the derivation of the flora of Hawaii, states: ". . . of the American element a far greater part of the species are only slightly distinct from their American relatives (probably

25 *Op. cit.*, p. 253.

26 Letter, August 23, 1949.

27 J. A. Jenkins, *The Origin of the Cultivated Tomato* (Economic Botany, vol. 2, pp. 379-392, 1948).

28 F. A. Fossberg, "Derivation of the Flora of the Hawaiian Islands" (in Elwood C. Zimmerman, *Insects of Hawaii*, vol. 1, Honolulu, 1948).

indicating geologically recent arrival) than is true for the other elements." The entire list of "cosmopolitan weeds" deserves to be studied from an ethnobotanical point of view.

THE CASE FOR MAIZE

There is evidence that suggests that America may not have been the home of maize (Zea mays), at least not in the sense in which we have been accustomed to consider it.

The origin of maize in America becomes more puzzling the more we learn about its history. It has in America no known close wild relative. For most domestic plants we are able to point to the area from which they probably come, point to related plants in that area, and usually designate a culture or people as the ancient possessors of the given plant. However, for maize we are unable to point either to a group of American plants from which maize, as we know it, can readily be derived, nor is there a cultural center to which we can point as being originally based on maize. Arracacha (*Arracacia xanthoriza*), manioc (*Manihot*), potatoes (*Solanum* sp.), quinoa (*Chenopodium*), amaranth (*Amaranthus*), chia (*Salvia chia*) are old American food stuffs with local centers of origin. Corn, however, recedes elusively from a Mexican-Guatemalan homeland, to Colombia, to the foothills east of the Andes, and has no clear home there. Though its home may be American, we do not know where in America that home may be. Though it may have a close relative in America, persistent search has failed to discover it.

Anderson found the maize growing in a remote oasis of northern Chile to be like ancient Peruvian (Nazca) but also in peculiar characters to resemble certain Oriental varieties.[29] This strange Chilean maize seems quite unlikely to have been carried by the Spanish into southeast Asia. Can it be, Anderson asks, that a primitive form of maize was carried across the Pacific long before 1500 AD? In light of the evidence presented above for yams, sweet potatoes, bottle gourds, and cotton, the possibility must be admitted. But, if so, which way?

One should note that if early transportation to Asia is involved, it could as well have been carried from Peru, where a type found also in Asia characterizes early levels. The presence of the bottle gourd in pre-Nazca levels indicates Asiatic contacts in very early times. However, the absence of maize from the early Peruvian levels containing cotton and bottle gourds seems to counter any claim for carriage of maize at that time to or from Peru.

29 Edgar Anderson, *A Variety of Maize from the Rio Loa* (Annals, Missouri Botanical Gardens, vol. 30, pp. 469-474, 1943); *What is Zea Mays? A Report of Progress* (Chronica-Botanica, vol. 9, pp. 88-92, 1945).

One of the arguments advanced against an early introduction of maize into southeast Asia is that if so introduced, it remained surprisingly localized. It apparently did not reach India or China where it might have attracted literary notice. It did not revolutionize agricultural practices in southeast Asia in the way that modern maize did when introduced after the seventeenth century. That the early maize that might have been carried was relatively poor stuff compared to the modern maize is, however, a significant fact bearing on the likelihood of its diffusing far and fast and assuming agricultural importance. Still, which way did it go? Where did it come from?

If there were a primitive, relatively unattractive, maize-like plant in southeast Asia, it might well have remained localized and have escaped literary notice. Had such a plant been introduced into America and here crossed with *Tripsacum*, a new type of maize may have arisen. This, and what Anderson proposes is, of course, only a variant of what Mangelsdorf has been suggesting. Mangelsdorf's recent report on maize from Bat Cave, New Mexico,[30] shows how poor a grain the early maize was in comparison to the material we now possess. The important role of *Tripsacum* hybridization in creating modern maize also becomes clearer as material like that from Bat Cave becomes available for study. The postulated minimum age of the earliest Bat Cave maize is 2500 BC. Yet if cotton and bottle gourds indicate Asiatic contacts with America at 3000 BC, then this date is not impossible in terms of an Asiatic origin. There are, however, some problems suggested by the absence of corn in certain early agricultural levels which should be examined here.

The presence at Bird's site in Peru of an agriculture containing such American crops as squash, chili peppers, and beans, combined with such Asiatic plants as bottle gourds and cotton but lacking corn, raises a number of questions.[31] If corn is ancient in South America and and especially in Peru and Colombia, why is it not present at this early site? The situation seems to be paralleled in the southeastern United States. In the Kentucky caves agricultural levels have been reported that contain squash, beans, sunflower, and other locally domesticated plants, and the bottle gourd.[32] The absence of maize is again striking. Is it possible that this indicates that maize was a relatively late introduction into America?

Birket-Smith has presented a mass of linguistic data tending to show that the

30 P. C. Mangelsdorf and C. E. Smith Jr., *New Archeological Evidence on Evolution in Maize* (Botanical Museum Leaflets, Harvard University, vol. 13, pp. 213-247, 1949).

31 Bird, *op. cit.*

32 V. H. Jones, "The Vegetal Remains of Newt Kash Hollow Shelter" (in W. S. Webb and W. D. Funkhouser, *Rock Shelters in Menifee County, Kentucky*, University of Kentucky Reports in Archeology and Anthropology, vol. 3, no. 4, 1936).

names for maize in much of South America and Middle America can be reduced to a very few basic roots.[33] This seems like evidence of the relative recency of maize in America rather than great antiquity. Birket-Smith argued from his linguistic data for a Colombian center of origin of American agriculture with corn as its basic crop. If corn is late in spreading through America, his data may mean no more than that that was the area into which maize was first introduced, possibly from overseas.

Birket-Smith's linguistic data seems to be in agreement with Bird's data. Both suggest a relatively late appearance of maize in South America.

It would be extremely interesting to have a parallel study of names for maize in North America. Would the linguistic evidence show greater diversity in North America than in South America and would this indicate greater antiquity? The Bat Cave evidence suggests early introduction of maize in North America. But a North American home for maize seems ruled out on botanical grounds, and North America is fairly well known botanically. Can it be that maize was introduced into Mexico or Guatemala from overseas at some very early date, even as early as 2500 BC?

A note on the Bate Cave date of 2500 BC may be made here. This is given as a minimal date, though the evidence for it seems to me rather shaky. Nevertheless, Bird has suggested a date for agricultural levels containing both Old World and New World domesticated plants at 3000 BC.[34] Volney Jones suggested a date of 2000 BC[35] for a local agriculture in the southeastern United States that contained the bottle gourd, an Old World plant. It seems significant that the three men working quite independently should suggest such dates for agricultural levels containing Old World plants or, in the case of maize, a plant of possible Old World origin. At the moment the data at hand suggest an earlier introduction of bottle gourd and cotton in Peru, and a possibly later introduction of maize perhaps into southern Mexico or Central America.

As these considerations bring out, the botanical relationships of maize deserve attention. Mangelsdorf has set out these relationships in detail, from which I abstract some pertinent points.[36] He suggests a derivation of maize from a wild form of pod corn which hypothetically lives or lived in the lowlands of South

33 K. Birket-Smith, *The Origin of Maize Cultivation* (Det. Kgl. Danske Videnskabernes Selskab, Historisk-Filologiske Meddeleser, vol. 29, no. 3, 1943).

34 *Op. cit.*, p. 335.

35 *Op. cit.*, p. 165.

36 P. C. Mangelsdorf, "The Origin and Evolution of Maize" (in *Advances in Genetics*, vol. 1, pp. 161-207, 1947). Anderson (*op. cit.*, 1945) first called attention to these botanical relationships.

America. Corn, he notes, belongs to the *Maydeae* tribe, which has eight genera. Five of these are Oriental: *Coix, Schlerachne, Polytoca, Chionachne, Trilobachne. Coix* is Job's Tears, a cultivated plant. All of these plants are native to the region of India, Burma, and the East Indies. It was this group that Anderson had in view when he suggested a possible Asiatic origin for maize or a plant that in America became maize.

The three American representatives of the *Maydeae* tribe are: *Zea, Euchlaena* (teosinte), and *Tripsacum*. Of these *Euchlaena* is a hybrid of the other two. *Tripsacum* is surely American. *Zea* is the plant whose origin is in question. We are left then, as Anderson noted, with but one ten-chromosomed *Maydeae* in America, all of its known relatives in the Old World, and the growing suspicion that this *Zea* may be an introduced plant.

Chromosome numbers then suggest that maize is more closely related to the Oriental *Maydeae* and to some of the *Andropoginae* than it is to *Tripsacum*. As Mangelsdorf has noted in discussing Anderson's suggestion that maize may be an amphidiploid hybrid (a plant resulting from a doubling of chromosomes during hybridization) of a five-chromosome sorghum and a five-chromosome species of *Coix:*

> Chromosome numbers alone, however, can be quite misleading as a criterion of relationship. . . . Failure of two species to hybridize does not necessarily prove a lack of close relationship since there are numerous morphological and physiological barriers, sometimes of a minor nature from the evolutionary standpoint, which can prevent crossing. So far as crossing experiments are a criterion of relationship they show that maize is quite closely related to teosinte, is somewhat more distantly related to *Tripsacum* and is only remotely related to the Oriental *Maydeae*. Indeed, it would appear that maize may be more closely related to some of the *Andropogineae,* such as sugar cane, than it is to its relatives among the Oriental *Maydeae.*[37]

In terms of Pacific relations it is important to note the presence of sugar cane and Job's Tears in the hands of Asiatic peoples and the Polynesians. Either one, if carried to America, could conceivably have formed the basis of a new grain ultimately to be known to us as maize. In terms of a world picture it should be noted that one of the African cultivated sorghums would be an equally likely possibility. Distances across the Atlantic are almost half those of the Pacific and the winds and currents are from Africa to America.

At the present moment, however, we can conclude nothing as to the home of maize except that an Asiatic or African home of a prototype can not be dismissed. The development of modern maize from some prototype of unknown origin

37 *Idem,* p. 166-168.

through a process involving hybridization with *Tripsacum* seems indicated. Evidence for great antiquity of corn in any culture in America has yet to be demonstrated.

A peculiar parallel can be drawn between maize and cotton. Hybridization in the New World has been postulated for both to have given rise to a new set of characters and led to the development of more useful plants than would otherwise have been possible. For cotton this hybridization was probably the result of introducing an Old World plant into the New World. It is possible that maize presents the same picture.

ON LOCALIZATION OF TRAITS AND DIRECTION OF ORIGIN

It seems important to note that the presence of a center of diversity of maize in Colombia and Peru can not be used against the possibility of African origin of the original maize plant. Neither for that matter can the presence of the yam in the Caribbean be used to argue against a possible Pacific derivation of that plant. Arguments have been leveled in the past against many traits for which possible transoceanic origins, usually Asian, have been advanced, on the basis that said traits were on the wrong side of the continent. It is interesting in attempting to see what might have happened in a remote and unknown past, to consider what we know to have occurred in the recent well known past.

The Spanish actions in America are probably the best illustrations that we possess of what could have happened in earlier times. There were relatively few Spanish. Yet in the period 1520 to 1540 they explored virtually the entire New World from Kansas to Argentina. Nor can all this be laid to force of arms. Cabeza de Vaca, shipwrecked and barely clad, was able to walk from somewhere on the Gulf Coast of the United States across the continent through all the intervening tribes, to the Gulf of California and thence down to the Spanish holdings in Mexico.

Of great interest is the location of the seats of Spanish settlement and power. They established themselves on the Mexican and Central American plateaus, in the irrigated valleys of Peru, the Mediterranean-type lands of Chile, and in the high plateaus of the Andes. They controlled the Argentine not from nearby Spain but from remote Lima. All foods and officers for Argentina came by way of Panama and were hauled over the Andes!

The Spaniards valued gold and silver and desired a large dependable labor supply. They went where these things were to be found. They also tended to avoid the wet and humid forested areas and settled on arid uplands or irrigated valleys

more like their own Spanish homeland. The presence of silver in the uplands reinforced this choice: probably the malaria and yellow fever that they introduced into the lowlands did likewise.

We can not then argue from the location of the center of dispersal of a trait, possibly introduced in the past, to the direction from which the introducers may have come. Invaders of the past might, as the Spaniards did, go great distances to settle in the area that provided them what they sought for or that most suited to their way of life.

Had peoples from southeast Asia ever reached Middle America, they might well have gone to considerable effort to find areas suitable to their cultural outlook. Their choice of location would depend almost entirely on who they were and their particular outlook toward their environment. Unless we know their culture, we cannot predict their choice of site. How then can we deny that peoples coming to America from some southeast Asian home might not go to the Caribbean islands or the Amazon basin to settle? How then can traits be denied as of possible Asiatic origin because they are on the wrong side of the American continents?

PLANT EVIDENCE ON THE NATURE OF ASIATIC-AMERICAN CONTACTS

The plants tell us something of the nature of the contacts that must have been made. It can not have been a fleeting contact. The exchange of crop plants involves reciprocal learning. Plants must be used to be appreciated. Methods of use, including cooking, planting, harvesting, and storage, are peculiar to each plant. Such learning involves intimate association and exchange of knowledge lasting at least through a growing season. *Argemone* suggests that the exchange of knowledge went beyond food plants into medicine and its associated magic and ritual. Another example is to be found in fish poisons. The *Lonchocarpus* of tropical America is not only used in identical fashion to the *Derris* of tropical Asia but appears botanically to be the same plant. Here, too, the evidence from names of plants is significant. That the sweet potato and the hibiscus were known in Polynesia and America by the same names speaks of intimate contacts.

Further, carrying plants across an ocean is not an easy undertaking. Vegetatively reproduced plants present special difficulties, but even seeds can stand only moderate amounts of moisture, heat, drought, or salinity. The seeds of cotton, for instance, rapidly lose their power to germinate when exposed to moist air. Quite expectably then, one finds suggestions in Polynesian accounts that in most cases plants were only carried by colonizing expeditions and usually when setting out for known lands. Thus we hear of canoes reaching New Zealand sending back

for useful plants. The sweet potato tubers brought by one boat, tradition says, were carried by a woman in a belt next to her body to keep them warm, protected, and viable.

One of the legends concerning the coconut's introduction to Hawaii also suggests the purposefulness of introductions and the failure to carry plants without definite planning.[38] Here is a paraphrase of the account: The coconut was brought by Apua and his elder brother Aukalenuiaiku. When they came to Hawaii the first time they did not bring any plants, for they thought all the plants of their country would be found in Hawaii. They went back to Kahiki and returned bringing various plants such as bananas and taro, and coconut, though only for the coconut is it specified that this was their introduction into Hawaii. Because of the greater distance and consequent difficulties it seems probable that only deliberately would plants be borne to or from America also. Only after careful preparation and planning could plants be carried so great a distance. This would be undertaken only if the presence of a land lacking the useful plants of the homeland of the explorers was known. This suggests purposeful, repeated voyages, not accidental, short-lived single contacts.

The plant evidence, then, is in total disagreement with the conclusion that, although contacts with America were made rarely and at long intervals, the net result was negligible.[39] Considering the time involved, the absence of written records, and the fact that only a few of the plants that suggest Asiatic-American contacts have been discussed here, the existence of this much evidence is striking. It is suggestive of long continued and important contacts of which we have in the plant evidence only a most incomplete and fortuitous record. Further, cultural items reinforce the judgment from plant evidence that the exchange of knowledge was no hurried and limited affair. Quipus common to Polynesia and Peru, the abacus in Peru and China, pachisi in Mexico and India are highly specific and very complex traits. The concepts surrounding their use, the understanding of their utility, and the fitting of them into an alien culture would not be easily and quickly done. A claim that such a cluster of complex cultural traits as these should be independently invented is not only mathematically and logically improbable, but indefensible in view of the demonstrable contacts of the people of America with peoples from the Pacific either possessing these traits or in contact with people who did.

To those who would object that the islands of the Pacific were not populated

38 *Fornander Collection of Hawaiian Antiquities and Folk Lore* (Memoirs, Bernice Pauahi Bishop Museum, vol. 5, pt. 3, 1919), pp. 590-592.

39 Dixon, *op. cit.*, p. 353; P. H. Buck, *Vikings of the Sunrise* (New York, 1938), p. 315.

until late, it must be said that the plant evidence is against them. Further the possibility exists that voyages were made to America from the Asiatic mainland with stopovers on the Pacific islands but withut a colonization of the islands. That a very early peopling of the islands did occur is, however, indicated. The Polynesians themselves state that they found some of the islands inhabited. In Hawaii there is geological evidence suggesting respectable evidence for man.[40]

History is against those who would object that boat culture was not sufficiently developed as early as the plant and cultural evidence suggests to be necessary. Developed navigation is indicated for the Aegean civilization before 3000 BC. About the same time people were apparently sailing from India to Arabia. Shortly thereafter people from the Mediterranean were sailing at least as far as Ireland and England. Pythias has left us a clear account of sailing so far north as to approach the arctic circle. The Norse and, even more strikingly, the Irish and the later Polynesian voyages show that oceanic voyages are entirely within the means of peoples equipped with boats of the type known to have existed by 3000 BC.

Clearer proof for contact between peoples from the Pacific with the peoples of Middle America could hardly be asked than that supplied by the sweet potato and by the hibiscus known as *maho*. Yams, *Argemone*, cotton, some of the so-called cosmopolitan weeds, and corn all suggest further such contacts, though with the weight of the evidence leading to a varying conclusion from highly probable to merely suggestive. The cumulative effect of the evidence in combina-

40 A. Fornander, *An Account of the Polynesian Race* (London, 1878), vol. 1, pp. 164-166. Because this interesting material has been so long overlooked I shall quote Fornander here in extenso.

"In 1822 the first wells were dug in the city of Honolulu. They passed through some eight or ten feet of surface loam and underlying volcanic sand, when a coral bed of some eight feet in thickness was encountered. . . . In this coral formation were found embedded a human skull and sundry human bones. . . .

"In 1858, in dredging the harbour of Honolulu, island of Oahu, near the new Esplanade, after scooping up and removing the mud and sand at the bottom of the harbour in about twenty feet of water, it was found that underneath this sand and mud was a pan of coral rock which it was necessary to break up and remove in order to obtain the required depth of water. This pan was of an average thickness of two feet, and beneath it was a thick couch of black volcanic sand, such as is found some four or five feet beneath the surface throughout the city, and evidently thrown out by the extinct crater of Punch-bowl-hill in some pre-traditional time. Embedded in this black sand, underneath the coral bed, was found the lower part or pointed end of an ancient spear or Ol, about three feet long. . . .

"Hawaiian traditions on Hawaiian soil . . . do not go back with any historical precision much more than twenty-eight generations from the present, or, say 840 years. Within that period the harbour and neighboring coast-line of Honolulu have remained nearly what they now are, nor has any subsidence sufficient to account for the formation of that coral bed beneath the city and its harbour, or of subsequent upheaval, or of any eruption from the Punch-bowl-hill crater, been retained on the memory of those twenty-eight generations."

tion with the other lines of evidence indicate that the parallel origin of civilization in the Old World and the New must be seriously questioned. Surely our picture of the origin and growth of the Middle American high cultures can no longer rest on the easy assumption of absence of extensive and ancient Old World contacts.

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A DRAFT MAP OF THE CULTURE AREAS OF ASIA

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IN A STUDY of culture areas made with an eye to some global classification scheme, I came upon the fact that no map at all of the culture areas of Asia existed.[1] Bacon indeed had brought Asia's confusion of ethnic and cultural patterns into provisional order. In 1945, she first outlined six main culture areas,[2] to which in 1946 she added four major areas of distinctive cultural blend.[3] The ten culture areas she thus distinguished were: 1. The Paleosiberian, 2. The Southwest Asian Sedentary, 3. The Pastoral Nomadic, 4. The Chinese Sedentary, 5. Southeast Asia and Indonesia, 6. The Primitive Nomadic, 7. Korean. 8. Japanese, 9. Indian or Hindu, and 10. Tibetan.[3a]

Kroeber has criticized her classification in a number of respects.[4] Many of the issues he has raised are beyond the scope of this paper, since it is not so much an attempt to make a new classification as it is to map existing ones. For my present purpose it is enough to notice that Kroeber accepts the classification of India, China, Japan, and—by implication—Southwest Asia as distinct culture areas, that he disclaims competence to treat of Tibet, reserves judgment on Korea, and concedes, regardless of qualifications and objections, that "in a mapped classification of Asiatic cultures, there is no recourse but to recognize the pastoral belt."[5]

Other observations of his do however call for some comment on my part. He points out that in Bacon's discussion "North Asia west of the Altai and Yenisei is unaccounted for, except for a reference to reindeer breeding."[6] Still, it seems to me at the very least open to question whether the Siberian reindeer-breeding peoples differ so markedly in their general culture from the surviving remnants of the hunting and fishing peoples that two separate culture areas need

1 Cf. M. J. Herskovits, *Man and His Works* (New York, 1949), p. 193.

2 Elizabeth Bacon and A. E. Hudson, "Asia (Ethnology)" (in *Encyclopaedia Britannica*, vol. 2, pp. 523-525, 1945).

3 Elizabeth Bacon, *A Preliminary Attempt to Determine the Culture Areas of Asia* (Southwestern Journal of Anthropology, vol. 2, pp. 117-132, 1946).

3a Since this article was set in type, a new classification of the culture areas of Asia has come to my notice, in H. H. Turney-High, *General Anthropology* (New York, 1949), p. 360.

4 A. L. Kroeber, *Culture Groupings in Asia* (Southwestern Journal of Anthropology, vol. 3, pp. 322-330, 1947).

5 *Idem*, p. 323.

6 *Idem*, p. 322.

to be distinguished. I have ventured to show as a single culture area all that northern portion of the Eurasiatic land mass in which the Siberian type of culture seems to be dominant. West of the Ob, the boundary shown is that of reindeer herding, which there coincides or nearly coincides with the linguistic boundaries and, as far as the White Sea, with that of the conical skin tent.[7] (I have, of course, ignored the changes made by recent Russian intrusion).

Kroeber's point that "if one proceeded to actual, accurate mapping of the area of pastoral nomadism, difficulties of representation at once appear"[8] is certainly well taken; difficulties do appear—nor do I pretend to have overcome them. I have simply shown as the Steppe culture area those parts of Central and Southwestern Asia which seem to be dominated by politically independent or at least autonomous pastoral nomadic tribes,[9] ignoring arbitrarily the sedentary settlements scattered among them; and in the same way I have shown as sedentary those areas which seem to be dominated by sedentary people, ignoring arbitrarily the pastoral fragments among them; nor have I attempted to distinguish between true nomads who raise no crops and semi-nomads who do some farming on the side.

Kroeber is inclined to classify Southeast Asia and Indonesia as a single area, ignoring the scattered primitive nomadic cultures or at best reducing them to a footnote. It seemed to me rather more useful to modify Bacon's scheme the other way, in favor of Heine-Geldern's three-fold classification.[10] I show a Southeast Asian Valley culture area (Heine-Geldern's "Civilizations" [Die Hochkulturen]), a Southeast Asian Hill culture area (Heine-Geldern's "Intermediate" [Die mittleren Kulturen]), and a Southeast Asian Marginal culture area (Bacon's "Primitive Nomadic"; Heine-Geldern's "Primitive" [Die primitiven Kulturen]). The Valley culture area shows the parts—largely valley or coastal—which are or have been the seat of sophisticated literate urban societies under strong Hindu, Chinese, or Moslem influence; the Hill culture area shows those—largely hill or inland—in which the original Malaysian cultural patterns appear more or less dominant still, and including border-line cases like the Menangkabau and the Shan states. The Marginal culture area, indicated on the map by heavy black dots, shows the remnants of primitive nomadic peoples like the Semang

7 See the linguistic, tribal, and trait distribution maps by Arthur Byhan in *Illustrierte Völkerkunde* (2nd ed., Georg Buschan, ed., Berlin, n. d.), vol. 2: Die Völker Asiens, Australiens und der Südseeinseln, pp. 288-289, 320-321.

8 Kroeber, *op. cit.*, pp. 323-324.

9 Here too I have ignored the changes made by recent European intrusion or influence.

10 *Illustrierte Völkerkunde*, vol. 2, pp. 689-968.

FIG. 1. Draft map of the culture areas of Asia.

Key to enclaves

Vertical hachures—Southwest Asian Sedentary
Diagonal hachures (NW-SE)—Steppe
Horizontal hachures—Southeast Asian Valley
Diagonal hachures (NE-SW)—Southeast Asian Hill
Solid black dots—Southeast Asian Marginal [add Palawan]

and the Aetas.[11] These three levels of Southeast Asian and Indonesian culture have been recognized—explicitly or implicitly—by others as well as Heine-Geldern.[12]

Perhaps such a mapping by levels might not be thought proper by those who follow strictly the original area study methods of Wissler and Kroeber. But it seems to me that in Southeast Asia, at least, the methods of Cooper and Steward are more useful,[13] as Kroeber himself would now seem to imply, when he bids us give primary consideration to historical developments where known, and further bids us notice the significance of political, religious, and literary aspects of culture as well as of economic and technological ones.[14] In so applying the method of Cooper and Steward to Southeast Asia, I thought it appropriate to include the Forest Veddas of Ceylon in the Marginal area, and the hill tribes of the Deccan, Bengal, Bihar, and Orissa in the Hill area.

In general, I think, the cultural patterns of Asia bear out Wissler's findings from American data on the role of the environment. Wissler showed that while the environment does not of itself either produce or determine cultural patterns, it does have a powerful influence on them; it not merely states the economic problems which people must solve but in each ecological region it tends to standardize some particular pattern which the people have chosen as a solution.[15] In sketching the cultural boundaries, I paid as much attention to ecological frontiers like those between taiga and steppeland, hill and valley, as I did to cultural frontiers like those between felt yurt and skin tent, flat-roofed house and gable-roofed house, or to linguistic frontiers like those between Santali and Bihari, Buryat and Tungus. Far more often than not, all three frontiers either coincided or approached each other very closely.

In preparing the map—which, as its title indicates, is a provisional draft offered for use in discussing or revising the existing culture area classifications—I

11 I have shown the Punan of Borneo, though their existence as a distinct culture group has been questioned by Fay-Cooper Cole, *The People of Malaysia* (New York, 1945), p. 199.

12 Kroeber, *op. cit.*, pp. 324-325; Harry Hoijer, "Peoples and Cultures of the Southwest Pacific" (in *The Southwest Pacific and the War*, Berkeley, 1944), p. 61; Raymond Kennedy, *Islands and Peoples of the Indies* (War Background Studies, Smithsonian Institution, no. 14, 1943), p. 13.

13 Cf. J. M. Cooper, *Areal and Temporal Aspects of Aboriginal South American Cultures* (Primitive Man, vol. 15, pp. 1-38, 1942), and J. H. Steward, "South American Cultures: an Interpretative Summary" (in *Handbook of South American Indians*, Bulletin, Bureau of American Ethnology, no. 143, vol. 5, pp. 669-772, 1949).

14 Kroeber, *op. cit.*, p. 330.

15 Clark Wissler, *The American Indian* (2nd ed., New York, 1922), pp. 372-374; cf. A. L. Kroeber, *Cultural and Natural Areas of Native North America* (University of California Publications in American Archaeology and Ethnology, vol. 38, 1939), p. 6.

have followed not only Bacon's published papers, but also my notes on her lectures,[16] and two multigraphed tribal maps she prepared for her students. I also leaned very heavily on the tribal and trait distribution maps in Buschan,[17] and found useful both Kennedy's tribal map of Indonesia,[18] and Finch and Trewartha's map of natural vegetation zones.[19]

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California

16 "Peoples of Asia," given at the University of California, Los Angeles Campus, in the Spring of 1949.

17 *Illustrierte Völkerkunde,* vol. 2, *passim* (see note 7 above).

18 Raymond Kennedy, "A Survey of Indonesian Civilization" (in *Studies in the Science of Society Presented to Albert Galloway Keller,* G. P. Murdock, ed., New Haven, 1937), p. 268.

19 As modified in *Atlas of World Maps* (Army Service Forces Manual M-101, Washington, 1943), p. 5.

HUPA SORCERY

WILLIAM J. WALLACE AND EDITH S. TAYLOR

INTRODUCTION

THE HUPA INDIANS of northwestern California entertain a strong belief that certain individuals possess supernatural or magical power which can be used to produce illness or death among fellow tribesmen. Bewitching is a practice open to anyone, male or female, who knows the correct procedure and is carried on by adults of all age groups. Men, however, seem to resort to it more often than women. A sorcerer usually directs his power against his own enemies, though he may occasionally, for a price, agree to attack another's foe. The power to bewitch is distinct from that of curing.[1]

MOTIVES FOR SORCERY

The motives for invoking black magic vary, though envy of possessions seems to be by far the most significant. Jealousy of success or possessions is strongly developed in Hupa culture, and an eminent or prosperous man is in constant danger of attack by an envious tribesman. A handsome well-liked child or the scion of an important family is also in continual peril and such children are often victimized by the parents of a less-endowed offspring.[2]

The Hupa are a sensitive people, easily insulted, and retaliation for a personal slight often takes the form of a supernatural attack rather than overt aggression or a demand for financial compensation. A quarrel, a prolonged enmity, a personal grudge, or a thwarted design may also motivate a vindictive person to use sorcery against his rival.

Social recognition or power cannot be obtained through sorcery. Great secrecy surrounds the evil art, and a practitioner dare not boast of his deeds for fear of retaliation by the kinsmen of his victims.

TYPES OF SORCERY

The Hupa recognize several distinct types of witchery, ranging from the utterance of a simple curse or formula to the utilization of a diabolic device by which

1 Shamans occasionally practise black magic and it is rumored that some do so in order to collect fees for curing the afflicted persons.

2 E. S. Curtis in *The North American Indian* (Norwood, Mass., 1924, vol. 13, p. 28) describes an incident of a jealous mother bewitching her son's playmate "because he was rich." A number of other cases of children being attacked by malevolent sorcerers are recalled by the Hupa. Another such incident is described later in this paper.

an invisible missile ("arrow") is shot into the victim's body. Common to all is the use of an incantation, the pronouncement of the name of the selected person, and the intrusion of a foreign object into his body. A practitioner of black magic need not encounter his victim face-to-face but can carry on his evil act from a distance.

Incantations. One of the most popular methods of sorcery is the recitation or chanting of certain words which in themselves have power to harm. Sometimes the incantation takes the form of imitative magic, the mere saying of the desired thing over and over again.[3] More often, however, the words are in the form of a traditional formula, the knowledge of which has been obtained from a parent or other close relative. The burning of angelica root or the blowing of angelica root powder off the palm of the hand normally accompanies the recital.

Certain ritual actions may occur in association with the use of a verbal charm. For example, a formula may be spoken over a stick which is then placed on the trail where, if the victim steps over it, he soon succumbs. A dread form, occasionally invoked after a killing, involves the slashing open of the corpse of the slain man and the mixing of some of his blood with the footprints of the killer while an incantation is recited.[4]

Evil spells must be narrated with fidelity and with a firm conviction in their power to produce the desired result. It is sometimes dangerous to employ them because they may react against the invoker, particularly if the intended victim possesses powerful counter-magic.

Contagious magic (choheksan "to put dirt in bad place"). A sorcerer can work evil by obtaining nail parings, hair, spittle, feces, or bits of clothing from the individual he wishes to harm. Such personal objects may be placed at a spot inhabited by a large number of rattlesnakes ("a rattlesnakes' den"), and the usual procedure of pronouncing the name of the person and uttering the correct formula followed. If a snake "eats" the offering, the victim immediately becomes ill and dies within the year. In a similar fashion the earth of a footprint may be taken to a rattlesnake, and the snake is told "what is wanted to happen."

An ill-wisher can also cause his personal enemy to be stricken by reciting a formula over his footprints and then burying close by a grave some of the earth

3 A simple malediction may be enough. For example, an aged person may curse a child for an impolite act such as running by on the trail without asking for permission. The youngster then falls ill because "old people have power and have it with them all the time."

4 This type of vengeance is vividly described in Robert Spott and A. L. Kroeber, *Yurok Narratives* (University of California Publications in American Archaeology and Ethnology, vol. 35, no. 9, 1942), pp. 185-189. It is said to have been employed by the Hupa against the Rekwoi Yurok during the Hupa-Rekwoi War and on several other occasions. This not only affects the killer but also his descendants for generations to come.

from the impressions. Hair or clothing inhumed near a grave will achieve the same result. An article of clothing placed in a pool of water will cause the owner to waste away as the fabric rots.

The Hupa are careful to dispose of their personal leavings so that they will not fall into the hands of an evil-doer. Children are instructed to dispose of all nail parings and hair-combings in the fire and to cover over spittle and feces. Adults carefully remove and put out of the way anything which might furnish a sorcerer with a starting point for his malpractices.[5]

"Poisons" (mil'kosa "put in mouth"). A number of "poisons" are administered by malevolent people, usually being placed in the selected victim's food or, in the case of a man, in his tobacco. One type is smeared upon an object which, when touched, causes injurious or deadly effects. Some of these are of plant origin, and a few may have actual lethal power, but most seem to depend upon the supernatural for their effectiveness.[6] A powder made from a portion of a human body, such as the finger bones, is used with fatal result, as are potions derived from snakes, lizards, frogs, and water dogs.[7] All are prepared in much the same fashion as curative medicines.

The recipient of a dose of poison loses appetite and gradually wastes away. Death is inevitable unless a shaman can diagnose and remove the source of the trouble. Some poisons are said not to take effect until a year later. Children are frequently victimized.[8] Poisons can be purchased for a high price though the trade in them is surrounded with great secrecy.

"Indian devil" (kitdongwe).[9] The most dreaded practitioner of black magic is

5 An amusing anecdote is told about an aged Hupa man who, while on an automobile ride with relatives, insisted upon carrying home his feces in a tin can.

6 Unidentified "weeds on the right side of the wind on Trinity Summit" are used, as are certain "fruits." None of these grow in Hoopa Valley. Tobacco growing on a grave is also regarded as a deadly poison.

7 All reptiles and amphibians are regarded as extremely poisonous. C. E. Woodruff in *Dances of the Hupa Indians* (American Anthropologist, vol. 1, pp. 53-61, 1892), pp. 53-56, mentions the use of powdered finger bones "collected in a certain way at a certain phase of the moon." He also refers to the use of of rattlesnake venom obtained by having the snake strike at a piece of meat which was then dried and powdered. This was sprinkled on the victim's bed and was absorbed by the skin, causing slow poisoning or possibly death.

8 One aged man told how he had been "poisoned" when he was twelve or thirteen years old. He became ill and "could not eat anything" and "threw up all his food." He continued to get worse until he was "just skin and bones." A famous Yurok doctor finally cured him by extracting a small frog, a lizard, and two small sticks ("which were used in mixing the poison"). The grandmother of his playmate introduced the "poison" into his food because "she was jealous."

9 The term "kitdongwe" refers to both the sorcerer and his supernatural device. No translation other than "Indian devil" could be obtained. "Kitdongwe sin sin" is the magical missile, "sin sin" being the usual designation for an intrusive disease-producing object or "pain." The

the kitdongwe or "Indian devil," who strikes down his fellows with a supernatural instrument made from a sharpened human bone or with a miniature bow shaped from a fragment of human rib and strung with sinew procured from the wrist of a recently interred corpse. Under the cover of darkness and with his face painted black, he approaches a dwelling and, after reciting a formula and uttering the name of his enemy, shoots an invisible projectile which enters his prey's body. It is not necessary for a kitdongwe to see his victim although he may remove a board from the side of the house or look down the smoke hole in order to make sure that his quarry is inside.[10] The sound of something moving about outside the dwelling, the excited barking of dogs, or other strange noises may indicate that an Indian devil is prowling around. Usually, however, he moves stealthily through the night, his presence revealed only by sparks flying up from his instrument of death which is carried under the arm.[11]

An Indian devil can change himself at will into a wolf or bear, and often prowls about in one of these disguises. If some one sees a track "that looks like a bear or wolf track but isn't," it is assumed that a sorcerer has passed that way. If pursued, a kitdongwe presses his contrivance to his breast and travels at great speed (". . . he can jump 'way off, like flying"). An Indian devil can only operate at night ("he cannot devil in the daytime").

Death usually follows an attack by a kitdongwe, the afflicted person being seized with a severe headache and dying within a few minutes. Occasionally, however, the victim falls into a coma or becomes violently ill. A powerful shaman can cure the sufferer if called in time, but the mystic projectile must be removed rapidly if he is to recover.

After a killing, an Indian devil "has to get his power back from the dead man." He must visit the grave and carry away the clothing and implements placed thereon to regain his capacity for evil doing. The relatives of the deceased may, if they are courageous, lie in wait in the cemetery for his visit and slay him. It is generally believed that a sorcerer must strengthen his power from time to time by obtaining portions of fresh corpses. Any disturbance of a recent burial is attributed to the actions of a kitdongwe and anyone observed near a fresh grave at night is suspected of being an Indian devil.

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kitdongwe are usually men, though during the past fifty years six or seven women have been suspected of this type of sorcery.

10 P. E. Goddard in *Life and Culture of the Hupa* (University of California Publications in American Archaeology and Ethnology, vol. 1, no. 1, 1903), pp. 64-65 states that the victim must be seen or his voice recognized. This was denied by informants interviewed.

11 The kitdongwe's implement constantly throws off sparks or "a blue light." Occasionally it may "cry like a baby," "hoot like an owl," or "make some other noise."

A chance nocturnal encounter with a kitdongwe usually results in bewitchment. Consequently most Hupa are reluctant to leave the house at night.[12] A brave man, whose ceremonial strength protects him from black magic, may occasionally lie in wait and seize a sorcerer and demand a huge ransom for his release.

A kitdongwe attempts to keep his heinous profession a secret, even to members of his own family.[13] His diabolic apparatus is carefully hidden when not in use. It may be concealed under a rock or in a hollow tree, or buried in the ground in a remote part of the forest. Even when completely covered, however, its presence is revealed at night by flashes of light. No attempt is made to steal it, because "it is dangerous to handle" and "no one dares to touch it." It seems doubtful that a kitdongwe succeeds in keeping his secret from his family, because his nocturnal absences and the various preparations necessary for bewitching would make any close relative suspicious.[14] The evil pursuits of an Indian devil are a constant threat to the lives of his relatives—perhaps an even greater menace to them than to outsiders. His lethal instrument is always a potential danger and may cause sickness or death within his family, or the sorcerer may deliberately exercise its dreaded force against his own kin.[15]

Occasionally two or three kitdongwe may work together as "partners" but there is no evidence that they ever all gather for a Witches Sabbath. Some villages are credited with harboring more Indian devils than others. Xonsadin, the northernmost Hupa village, has long had such a reputation. The Southfork Creek people, closely related to the Hupa in language and culture, are regarded as expert kitdongwe.

DIAGNOSIS AND CURE

Sorcery is believed to be the cause of much, though not all nor even the ma-

12 A short poem, "Bedtime in Hoopa," by Henry A. Kendal (*Saga of the Redwoods and Other Writings* 1942, p. 17), gives some indication of the nightly fear of the kitdongwe.

Screech owl holler, scary sound,
Injun Devil traipsin' round,
Runs like lightnin' here and there,
Mebbe wolf and mebbe bear.

Dead man's rib and bowstring twist
Of sinews from a dead man's wrist,
Unseen arrows, pizen breath.
Make you sick or bring you death.

Sit together, hide your heads,
Look for pizen in the beds,
Shut the door, the windows tight,
Injun Devil round all night.

13 Generally, however, an aging sorcerer imparts his esoteric knowledge and equipment to a close relative when death is imminent.

14 Just as for any ritual act, a sorcerer must abstain from sexual relations, eat alone, and follow certain other prescribed behavior before practising black magic.

15 It is said that the kitdongwe soon "lose all human feelings" and "even kill close relatives."

jority of, disease and death. Natural causes for illness or demise are recognized, but if a sickness is long-continued or unusual in any way, the explanation is sought in the machinations of a malevolent human agent. Sudden seizures or deaths are interpreted in the same way. When several well-known men die in succession, suspicion is aroused that they are victims of black magic. A shaman called in to treat a patient will often ascribe the affliction to a supernatural object or "pain" sent by a sorcerer, and the diagnosis is readily accepted by the patient and his family.

A "pain" introduced by a vindictive person is more troublesome to remove than one caught accidentally from the air in the usual fashion, and a shaman has difficulty in extracting it. A shaman may fail to remove the disease-causing object if it has been in the sufferer's body too long or because he lacks the necessary power. If the "pain" is removed, the shaman carries it outside and places it on a block of wood, and covers it with a small basket. Most of the people of the village gather around, and the shaman calls out the name of a person suspected of causing the disease and lifts the basket. If the object is still there, that person is innocent, and it is again covered and another name is called. This continues until the disease-object disappears and the one whose name is last called is assumed to be the guilty party ("when the doctor hits the right one, the pain goes away"). The accused is given a chance to confess: his admission will hasten the sufferer's recovery. If he refuses to confess or protests innocence, he may be beaten or killed by relatives of the patient.

PREVALENCE AND EFFECTIVENESS OF SORCERY

It is difficult to determine with any exactness the frequency of witchcraft among the Hupa because it is never practised openly and it is a subject which many people are unwilling to discuss. The Hupa are not obsessed with ideas of sorcery, though bewitching is feared and both men and women manifest anxiety concerning it.[16] Though witchcraft is not a commonplace phenomenon, a fairly large number of incidents involving witchcraft in recent generations are recalled.[17]

16 There is some indication of a slight upsurge in the black art in the period following white contact. For example, an outbreak of witch fear occurred when the Saia Indians, Athabascan-speaking peoples from the Mad River and tributaries of the Eel River, were brought to the Hoopa Reservation in 1868. The Saia are said to have tattooed their entire faces in a manner similar to the face painting of kitdongwe and were reputed to be "strong Indian devils."

17 W. J. Wallace, *Hupa Narrative Tales* (Journal of American Folklore, vol. 61, pp. 345-355, 1948), gives several more or less typical accounts of sorcery. A great many more were collected.

Hupa mythology is not overburdened with tales or incidents of witchcraft.[18]

It is equally hard to assess the efficacy of witchery in actually attaining its purpose. Direct poisoning may be effective when toxic plants are employed but other forms of sorcery, with the possible exception of the operations of the kitdongwe, are probably ineffectual, because, for the most part, the victim does not know or even suspect the sorcerer's intention. However, should a person fall ill and the affliction be diagnosed as the result of black magic, it may become effective psychologically. A patient who believes that he is under the spell of a sorcerer may wither away and die without any real or apparent physical reason.

PROTECTION AGAINST SORCERY

There is no elaboration of techniques for countering the attacks of sorcerers, though many precautionary measures are taken. Children are cautioned against accepting food from others; personal offal is carefully disposed of; unnecessary trips into the night are avoided; a man is careful not to offend anyone unnecessarily lest animosities be stirred up which would lead to black magic being used against him; contacts with individuals suspected of controlling diabolic power are avoided where possible.[19] In addition, leaves of the pepperwood tree or bits of angelica root are carried to ward off evil. A few people own protective songs or formulas. None of these is a sure defense, however.

A man who has obtained strong supernatural "power" through fasting, hardships, and religious contemplation is relatively immune from an attack. It is also believed that a person who has led an exemplary ("good") life and has participated in the great group ceremonials has a certain amount of security against the antagonistic actions of malevolent people.

DETECTION AND PUNISHMENT OF SORCERERS

A sorcerer is occasionally detected committing an evil act but more often his identity is revealed by other means. The name-calling test carried on by a shaman results in detection, or a powerful shaman may perceive the evil-doer in a revelation which occurs during the regular ritual of curing. The identity of a

18 There are a few tales of witchcraft. Two myths in P. E. Goddard's *Hupa Texts* (University of California Publications in American Archaeology and Ethnology, vol. 1, no. 2, 1904) "Xonsadin Young Man" and "Gooseberry-place Brush Dance," (pp. 177-178, 182-183) are concerned with kitdongwe. Edward Sapir also recorded the former and two other tales involving black magic—"A Blaspheming Village" and "Salmon Trout is Doctored by Blue Jay and Hummingbird" (Edward Sapir, *Hupa Myths, Formulae, and Ethnologic Narratives,* manuscript).

19 After battle, warriors took precautions to avoid revenge magic sent by kin of slain enemies (W. J. Wallace, *Hupa Warfare,* Southwest Museum Leaflets, no. 23, 1949, p. 9).

sorcerer may also be divulged to his victim. The sick man usually suspects one of his enemies and should this individual appear to him in a dream it is accepted as proof that he is the malefactor.

A person may inadvertently fall under suspicion of practising witchcraft. A threat against an individual who subsequently falls ill, perhaps months or even years later, may be remembered and used as an indictment against him. Also anyone whose habits are unusual or irregular ("who acts suspicious—not right") may acquire a reputation for witchery, particularly if seen wandering about at night or if observed doing strange things out in the forest. A close relative of a known sorcerer is always regarded with distrust because of the possibility that knowledge of the black art may have been passed on to him. Several individuals in each village are usually looked upon with suspicion and avoided when possible. Quite often these are ineffectual old people, poverty-stricken men or women who live alone and have few relatives.

An accused sorcerer is usually given an opportunity to confess his odious crime because, as already mentioned, an admission of guilt aids the recovery of the sufferer. The suspect often "confesses" perhaps because he fears violence if he refuses, or because the suggestion of the idea immediately produces a sense of reality, so that he is quite incapable of distinguishing what he has merely thought from what he has actually done. An individual may acknowledge the misdeed because he enjoys the attention or recognition. Torturing a suspect or submitting him to an ordeal is not known. If the afflicted recovers after an admission of sin, the sorcerer is usually not punished. However, if the suspect disavows guilt or if the patient dies following a confession, personal revenge is taken by the relatives of the victim. In cases of death from black magic retribution is swift, and mere suspicion is usually enough to bring reprisal. The normal punishment is death, and is in no way mitigated when the accused happens to be a woman. The suspect is quickly executed by being shot from ambush with bow and arrow, or beaten to death with clubs and stones. Hanging is not practised, and it seems doubtful that another sorcerer is ever hired to kill a suspect by witchcraft.

Relatives are not likely to stand solidly behind a kinsman accused of witchery. A family normally defends its members against outsiders, but in a case of witchcraft, the sorcerer's kin faces an aroused population, and they themselves consider sorcery as an abhorrent crime.[20] Compensation, which is regularly demanded for a murdered kinsman, is usually not requested for an executed sorcery suspect because the burden of proof is placed upon the accused's relatives.

20 If, however, they believe the charges to be false, they may defend the suspect and if he is slain they will seek revenge, thus touching off a blood feud.

There is no special type of burial for a sorcerer and the soul of a deceased malefactor does not go to a different hereafter but to the underworld which is the final abode of the souls of ordinary men.

FUNCTION AND RELATIONSHIP OF HUPA SORCERY

Witchcraft, although it is always looked upon as utterly bad and as an evil which cannot be sanctioned, nonethless performs several distinct functions in Hupa culture. It is a factor which operates to influence and channelize human conduct. A Hupa is careful not to offend anyone unnecessarily for fear that black magic may be used against him. He also seeks to avoid suspicion of being an evil practitioner by conforming to established norms of overt conduct instead of indulging in aberrant or idiosyncratic behavior.

Sorcery is also an outlet for an expression of hostility which cannot be exhibited directly.[21] Insults, threats, or actual physical violence are avoided because they must all be compensated by money payments, and the average Hupa is extremely reluctant to squander any of his wealth in this manner. If a man cannot engage in overt aggression in face-to-face situations, he can, at least, injure his rival by sorcery. Thus he can give vent to his anger or jealousy with some feeling of satisfaction and without fear of financial loss or other retaliation.

The Hupa beliefs and usages connected with witchcraft are shared with minor variation by their close neighbors, the Yurok and Karok, and by the more distant Tolowa. Undoubtedly the practices of other tribes in this general area of northwestern California and southwestern Oregon are similar but detailed data for them are lacking. Many of the traits seem to have a wide distribution in western North America, but more information on witchcraft is needed before any definitive statements on distributions and relationships can be made because facts on witchcraft in many tribes have not been collected as yet. There are some traits shared by the Hupa with their fellow Athabascan-speakers in the Southwest, the Apache and Navaho, but these are probably general throughout the western part of the continent and do not represent a specific Athabascan heritage.[22]

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21 A. I. Hallowell in his article *Aggression in Salteaux Society* (Psychiatry, vol. 13, pp. 404-405, 1940) stresses the function of sorcery as a means of release for hostility.

22 Clyde Kluckhohn in *Navaho Witchcraft* (Papers, Peabody Museum of American Archaeology and Ethnology, vol. 22, no. 2, 1944), after a brief examination of the materials on the Athabascan-speakers of Canada and the Pacific Coast, suggests that their witchcraft beliefs resemble those of the Navaho considerably less than they did those of neighboring peoples speaking languages of other families. He failed to find any elements which could be regarded as "general Athabascan."

GENETICS VS. PHYSICAL ANTHROPOLOGY IN DETERMINING RACIAL TYPES

CHANDLER W. ROWE

IN A RECENT ARTICLE[1] W. C. Boyd discusses physical anthropology and genetics and concludes:

> The whole elaborate structure of anthropometry was never consistent with its own basic assumptions, and was destined ultimately to collapse of its own weight, even if a knowledge of genetics had not begun to diffuse . . . from the laboratories to the museums.[2]

It is apparent that Boyd has little faith in the traditional classification of Man into racial groups. What are Boyd's objections to the present racial classification? To begin with Boyd sets up certain requirements which he feels are necessary for a valuable criterion of classification. Such criteria, to be of value, must meet the following requirements:[3]

1. The criteria must be objective.
2. They must not be subject to too much modification by the environment.
3. They must be determined by one, or a small number of genes.
4. They must be non-adaptive; that is, they must not have any great selective value in evolution.
5. They must not mutate at too high a rate.

He then considers such physical characteristics as stature, measurements on the skull and other parts of the body, pigmentation of the skin, hair, and eyes, physical characteristics of the hair, blood groups, and taste reaction to phenyl-thio-carbamide and concludes, "It would seem, then, that only blood groups and possibly taste reaction to phenyl-thio-carbamide can be expected to meet our five stipulations."[4]

Boyd's criticism of the physical characteristics may be illustrated by an example. He points out that the cephalic index is hereditary to some degree but

1 W. C. Boyd, *The Use of Genetically Determined Characters, Especially Serological Factors Such as Rh, in Physical Anthropology* (Southwestern Journal of Anthropology, vol. 3, pp. 32-49, 1947).

2 *Idem*, p. 49.

3 W. C. Boyd, *Critique of Methods of Classifying Mankind* (American Journal of Physical Anthropology, vol. 27, pp. 333-364, 1940), p. 334.

4 *Idem*, p. 337.

is determined by a number of genes. It does not, therefore, conform to the third requirement stated above. He goes on to quote Baur, Fischer, and Lenz as mentioning that even when two heads coincide in length, their apparent similarity may actually be due to difference in bony structure. The recognition of this difference by physical anthropologists, it may be noted, is one reason for the increased use of endocranial measurements—to allow for the differences in the thickness of bone on various skulls. Even so, no physical anthropologist would compare the cranial measurements of, for example, an Australian aboriginal and a European White without mentioning the greater development of the supraorbital ridges, and therefore the greater thickness of bone, on the Australian.

Boyd further states that there is some evidence that headform is adaptive. This he puts down to selection as the primary agency which brought about the increase in brachycephaly in Europe. He brings in also the effect of environment on headform as derived from the work of Boas[5] and Shapiro.[6] But it may be asked, has anyone proved that headform is an adaptive characteristic? The studies of Boas and Shapiro do not show that headform changes when other physical characteristics do not. Immigrant children who are raised under better conditions than their parents may grow taller, in which case the change in headform might be the result of their increase in stature. But can it be shown that the environment will influence headform directly without the presence of accompanying changes in other physical characteristics?

Mention is also made of the effect of climate on the nasal index as another example of environmental influence on physical characters. Boyd cites here the work of Thomson and Buxton[7] and Davies.[8] The theory, briefly, is that the long, narrow nose is better adapted to breathing air of the cold regions and therefore is most common in such climates, while the wide nose with flaring nostrils is better adapted to and so is more frequent in the warm, moist climates. At least a shadow of a doubt can be thrown on this theory when we consider that Neanderthal man lived in Europe when the climate was quite cold due to the presence of glacial ice, and that a large broad nose with wide nostrils is the shape generally considered to be typical of the Neanderthal forms. The noseform common among the Eskimo further discounts the idea.

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5 F. Boas, *Changes in Bodily Form of Descendants of Immigrants* (Senate Document, 61st Congress, 2nd Session, no. 208, Washington, 1911; same, New York, 1912).

6 H. L. Shapiro, *Migration and Environment* (New York, 1939).

7 A. Thomson and L. H. D. Buxton, *Man's Nasal Index in Relation to Certain Climatic Conditions* (Journal, Royal Anthropological Institute, vol. 53, pp. 92-122, 1923).

8 A. Davies, *A Resurvey of the Morphology of the Nose in Relation to Climate* (Journal, Royal Anthropological Institute, vol. 62, pp. 337-359, 1932).

The evidence which Boyd brings to bear on such physical characteristics as mentioned above and the conclusions which he draws can also, he says, be extended to cover any other such characteristics as may be suggested. Boyd, then, would discount as useless those physical characteristics which are used today for racial classification. By what means and with what factors would he classify Man into "races"? Of primary importance, he feels, are the known blood groups since

> blood groups are characteristics genetically determined by known mechanism, absolutely objective in character, absolutely unaffected by environment, not subject to mutation at any rapid rate and, so far as we are able to discover by extensive investigation, non-adaptive.[9]

What Boyd is doing here is using only one criterion for racial classification, a practice which he condemns by saying,

> . . . one of the first preconceptions which most laymen must get rid of is the idea that some one physical characteristic will be enough to differentiate all the races, one from another.[10]

In spite of this inconsistency, Boyd goes on to plot the frequency of occurrence of various blood types for different racial groups, using the classical Landsteiner blood groups O, A, B, and AB; the M, N types of Landsteiner and Levine; and recognition of the Rh series of genes on the part of Landsteiner, Taylor, Weiner, Race, and others. In his Table 1,[11] Boyd illustrates the racial distribution of the Rh blood types. That table points out that a high incidence of Rh^0 exists in the Negroid groups, that the Rh negative gene is practically absent from the Mongoloid and Pacific peoples, and that the Rh^s gene is higher in the Australians and Mongoloids than in any other group. The question immediately arises: How does he know which are the Negroid and which are the Mongoloid groups if his classification is based on blood groups alone? It seems apparent that Boyd has picked his samples by utilizing the traditional classification of races, which he condemns, and has used blood groups to substantiate the differences between them.

Earlier in the study of blood groups it was stated that all the North American Indians belong to blood group 0. This statement was made on the basis of a study of American Indian groups in which the sample was too small. Later when a larger sample was tested, it was found that some North American Indian groups had more blood group A than any peoples of Europe. Boyd is aware of this and warns against over-simplification of the blood group method of classification.

9 Boyd, *Critique of Methods*, p. 337.
10 Boyd, *Use of Genetically Determined Characters*, p. 35.
11 *Idem*, p. 43.

Yet his conclusions based on the distribution of the Rh genes, are drawn from samples smaller than the ones which indicated that all North American Indians belonged to blood group 0. Testing of larger samples may well prove his conclusions erroneous.

Boyd has done considerable work with blood groups and with the Rh series in particular. He has been exceedingly vigorous in his work, but apparently this vigor has been concerned primarily with taxonomy. His energies have been directed toward establishing a classification rather than a description of races of mankind. This classification in itself seems to be his aim. The physical anthropologist, however, looks at classification as a basis from which to work on other problems.

Boyd does not stand alone in his criticism of the present racial classification. He has staunch support for the genetic approach in F. M. Ashley Montagu, among others, who says that in order to understand the differences among the groups of mankind, "What must be studied are the frequencies with which the genes occur in different groups or populations."[12] Montagu goes on to say

> . . . the indictment against the anthropological concept of race is (1) that it is artificial, (2) that it does not correspond to the facts, (3) that it leads to confusion and perpetuation of error and finally, that for all these reasons it is meaningless, or rather, more accurately, such meaning as it possesses is false. It were better that the term, being so weighted down with false meaning, were dropped altogether than that any attempt should be made to give it a new meaning.[13]

Montagu's concern with the term "race" apparently stems from the fact that the layman has confused not so much the biological with the religious or linguistic, i. e. cultural aspects of Man, but that the connotation of superiority and inferiority of one "race" to another has been placed upon the term. To avoid this, he substitutes a new term, "ethnic group," which is a group within the human race. Montagu's desire to drop a term which has come to have unscientific implications in popular thinking is valid and one which is understandable to anthropologists in general. However, there is no certainty that the term "ethnic group" or any other substitute for the term "race" would not have the same connotation carried over to it. Aside from this purpose, Montagu apparently feels that the genetic concept of race is the most accurate, and he, therefore, denies the present method of racial classification. It is interesting to note, however, that in his introductory

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12 F. M. A. Montagu, *Man's Most Dangerous Myth: the Fallacy of Race* (New York, 1942), p. 35.

13 *Idem*, p. 36.

book[14] he uses a racial classification of mankind that is surprisingly similar to the one used by Hooton,[15] a classification which is based on the traditional method used by physical anthropologists—the use of measurements and observations on the complexes of physical characteristics in man.

Other geneticists have expressed opinions with regard to the present racial groupings, but few take as strong a stand against it as does Boyd. Dunn and Dobzhansky also survey physical anthropology and genetics,[16] but they seem to feel that the physical anthropologist's method of combining all the measurements and observations of persons in a certain group and calculating the average for each physical characteristic is a valid one for determining the set of average characteristics for that group. However, they say, the various races such as Negroid, Nordic, and Alpine differ as groups, and the difficulty arises when an attempt is made to put individuals into one or the other of these groups, particularly in the classification of individuals into the Nordic, Alpine, and Mediterranean categories. They say,

> The averages may describe very well the ideal Nordic or Alpine, but ideals and averages are abstractions, and it is just a lucky accident if the person whose race we wish to determine happens to have all his traits coincide exactly with the averages for any one race.[17] [And further] If we make many measurements and calculate averages for all of them [Nordic, Alpine, and Mediterranean] we may find ourselves in the predicament that no actually existing man or woman of any race anywhere conforms to the race ideal.[18]

That Dunn and Dobzhansky do not hold entirely with the present racial classification is clearly shown when they indicate that the old race notion was mistaken in that race was believed to be a "community of blood." Their feeling is that if this were true, the racial idea which was derived by averaging the characteristics of a group which had intermarried for many generations would have some meaning. The present-day physical anthropologists understand that there are problems involved in dealing with abstract types, but most of them also know that the race from which a person comes only partly determines his heredity. The processes involved in the transmission of many of an individual's physical characters are not known even to geneticists. Dunn and Dobzhansky utilize much of the terminology and some of the concepts of the modern racial classification. Their pri-

14 M. F. A. Montagu, *An Introduction to Physical Anthropology* (Springfield, Illinois, 1945), fig. 18.

15 E. Hooton, *Up From the Ape* (rev. ed., 1933), fig. 57.

16 L. Dunn and Th. Dobzhansky, *Heredity, Race and Society* (New York, 1946).

17 *Idem*, p. 97.

18 *Ibid.*

mary objection is that it holds for groups only, and they look to the genetic approach for the possible solution to the problem. There are many other instances where geneticists have continued to carry on their work using as a basis the present-day classification based upon anthropometry and observation, admitting, however, that the processes involved in the inheritance of these measurable and observable characters are not fully known.

In all fairness to Boyd and those who share his views, it must be admitted that not all the anthropometric data are worthwhile. In some cases measurements have been taken and recorded without apparent reason other than to be taking measurements. But by and large the mass of anthropometric data with their accompanying observations have proved to be extremely valuable, and the resulting classification cannot be lightly dismissed by saying that it "is not consistent with its own basic assumptions."

What are the physical anthropologists' views on the present racial classification? The majority of them, of course, hold that it is workable, but that it is by no means entirely satisfactory, that there are disadvantages and inadequacies.

Hooton[19] sums up the controversy between the environmentalistic school and the strict hereditarians by saying,

> The hereditarians judge too much from appearances, whereas the environmentalists are apparently committed to the view that appearances are invariably deceitful. . . . In our present state of knowledge we cannot affirm with absolute certainty that any single bodily character is inherited generation after generation without modification, and we are equally unable to point to a single bodily character, variations of which can be definitely proven as adaptive or environmentally actuated.

He goes on to say that

> nevertheless, we are on comparatively firm ground when we assert that a combination of several selected physical characters in an individual usually gives us a clue to his racial ancestry.[20]

Hooton contends that a single physical character may turn up in an individual in more than one race, but that a combination of characters such as black wooly hair, black skin, broad nose, prognathism, and everted lips, could only be due to race inheritance. He then discusses the physical status of the American Indian and finds the following physical characters to exhibit relatively small variability:

1. *Hair color*—almost invariably black.

19 E. Hooton, "Racial Types in America and Their Relations to Old World Types" (in *The American Aborigines,* D. Jenness, ed., Toronto, 1933).

20 *Idem,* p. 135.

2. *Eye color*—nearly always dark.

3. *Hair form and texture*—prevailingly straight and coarse.

4. *Quantity and distribution of body hair*—head abundant, beard and body hair very sparse.

5. *Skin color*—light yellow-brown through darker yellow-brown and red-brown.

6. *Malar size and projection*—large to very large. Considerable lateral and frontal projection.

7. *Prognathism*—moderate.

8. *Chin prominence*—sub-medium development.

9. *Size of face*—always large.[21]

The following features have relatively large variability:

1. *Cephalic index and headform*—varies from extreme dolichocephaly to hyperbrachycephaly.

2. *Nose form*—from platyrrhiny to leptorrhiny.

3. *Eye folds*—majority lack the Mongoloid fold. The external fold is present in some old males.

4. *Facial index*—varies from chamaeprosopic to leptoprosopic.

5. *Lip form*—vary from very thin to more than average thickness.

6. *Stature*—varies from 153 cm to about 174 cm.

7. *Body build*—the differences are associated with the differences in stature.[22]

The above data having been derived from measurements and observations on a long series of individuals, these conclusions can be considered as having a high degree of validity. Hooton then points out that the crux of the problem is not the distribution or range of variation of these physical characters taken singly, but the combination of these features into distinct hereditary types.

Krogman,[23] in summarizing the problems involved in racial classification from the point of view of the physical anthropologist, sets up six points which illustrate the facts concerning race about which little or nothing is known. The first point is that the physical anthropologists are not sure what constitutes a biological race in man. "At species level distinctions are quite clear; below that they are dim in the haze of variability."[24] This point of view is also held by Dahlberg,[25] who

21 *Idem*, p. 136.

22 *Idem*, p. 138.

23 W. M. Krogman, *What We Do Not Know About Race* (Scientific Monthly, vol. 57, pp. 97-104, 1943).

24 *Idem*, p. 99.

25 G. Dahlberg, *Race, Reason and Rubbish* (London, 1942).

says that ". . . the number of hereditary characters which distinguish two closely related species is generally greater than the number that distinguish two races within either of them. . . ." Species, Dahlberg says, are the smallest groups which have sharp boundaries. Krogman's second point, or "don't" as he calls it, is that we are not sure how stocks and races arose. We have not gathered sufficient information on fossil finds to tell us when in the process of human evolution the races of Man became differentiated. The third "don't" recognizes the fact that too little is known concerning the processes of heredity in Man. The fourth "don't" follows the third in stating that although there is a hereditary basis, it is not known which traits are stable and which are modifiable. The fifth "don't" indicates the meager knowledge concerning the physiology of race-types, and the sixth point is that no characteristics are known to be superior or inferior to each other.

That the physical anthropologists are not blind to the disadvantages and limitations of the present classification is apparent from the statements above. That Boyd has been unjust in his criticisms[26] is evidenced by Krogman when he concludes,

> The term race as we use it today is a recognition that group differences do in fact exist. It does not imply, scientifically and biologically, a homogeneity such as demanded by geneticists. When our knowledge of human heredity enables us to classify the peoples of the world genotypically we will gladly accept that classification—we will substitute it for the one we now have. Until then, and with full and complete recognition of all of its many inadequacies we will use the system at hand.[27]

The geneticists have rejected the present racial classification, apparently for two reasons. The first is that it is inadequate. The second is that it has been established through the utilization of physical characters whose hereditary processes are little known, which are adaptive to a degree, and which have selective advantage or disadvantage. What, then, have the geneticists to offer in the way of racial characters which are non-adaptive, not subject to modification, and about which enough of the hereditary process is known to make them useful as racial criteria? Most geneticists freely admit that the distributions of only a small number of human genes have been studied, and that even these have been studied only to a limited extent. However, some information can be presented from the data collected.

Red-green colorblindness is known to have an hereditary basis. It is inherited as a sex-linked recessive; that is, the gene for colorblindness (cb) is carried on

26 Boyd, *Critique of Methods*, p. 338.
27 Krogman, *op. cit.*, p. 104.

the X-chromosome and does not express itself unless the gene for normal vision (Cb) is absent. Colorblindness is also known to be more prevalent among men than among women. Strandskov figures a frequency of colorblindness in various populations[28] and draws certain conclusions as to frequency in various races. Briefly, they are as follows: the frequency among the males of Norwegian and German populations and for the unselected United States Caucasoids is about 8 percent. The colorblind gene (cb) is only half as common as this (or about 4 percent) among the United States Negroes; thus there are half as many colorblind Negroes as Whites in the United States. Among the Chinese, the frequency is about halfway between the United States Negroes and Whites. Among the American Indians the frequency is the lowest found in any group.

Ability to taste phenyl-thio-carbamide is another characteristic about which the process of inheritance is known. It has been discovered that 70 per cent of American people are "tasters" and 30 percent are "non-tasters." It is also known that this characteristic, the ability to taste PTC, has familial incidence and is inherited as an autosomal recessive. Strandskov figures the frequency of "tasters" and "non-tasters" in various populations.[29] It is evident from this table that there are approximately the same number of "tasters" in the Negroid as in the Caucasoid populations tested. There are considerably more among the Mongoloids. The American Indians have about the same frequency as the Japanese and Chinese populations in terms of percentage. (There is, however, a considerable difference in the size of the samples for the various populations.)

Pitch of Voice is also inherited, but it is doubtful that it would have much value as a racial criterion.

The frequency of the secreting factor has been reported on by Schiff.[30] "Secretors" are those who have present in the saliva, gastric juices, or other bodily fluids the blood group specific A and B, or the substances which characterize the blood groups. Those individuals whose saliva does not have the respective group substances are called "non-secretors." Schiff has tested Whites and Negroes as to frequency of "secretors" and "non-secretors" and finds that there is a greater frequency of "non-secretors" among the Negroes. His sampling is open to criticism, however, since he has tested a considerably smaller number of Negroes than Whites. In addition, his total series for both Negroes and Whites is only some 650 individuals.

28 H. Strandskov, *The Distribution of Human Genes* (Scientific Monthly, vol. 52, pp. 203-215, 1941), table 1, p. 206.

29 *Idem*, table 2, p. 207.

30 F. Schiff, *Racial Differences in Frequency of the Secreting Factor* (American Journal of Physical Anthropology, vol. 27, pp. 255-262, 1940).

Gates[31] has discussed certain anthropological factors and their inheritance in considerably more detail than can be treated in this paper. Briefly, his findings include the following:

Stature—stature within a race is inherited. It is controlled by multiple factors, and in extreme statures endocrine differences are involved which obscure the genic effects influencing the size of the body directly.

Nose—a narrow nose is dominant over a broad nose.

Jaw—there is some evidence that jaw form is inherited, for example, the Hapsburg jaw and lip.

Hair form—straight hair is recessive and curly hair is dominant. Although wavy hair was formerly thought to be a heterozygous condition between curly and straight, it is believed now that wavy and curly are separate genes; the dominant relations between the two have not as yet been discovered.

Head shape—this character is relatively stable. Brachycephaly is dominant over dolichocephaly. It is known that genetic segregation of head shape occurs in crosses and that there are both shape and size factors which affect head shape, and that the former are dominant over the latter.

Sacral spot—the sacral or "Mongoloid" spot is apparently dominant in some racial crosses and recessive in others. It has been found rarely in Western Europeans, universal in the Japanese and occurs in more than half of the Negro infants tested on the Ivory Coast of West Africa. The latter figure may be too low since the spot is hard to see on the Negro because of deeper pigmentation. The dominance, Gates says, is probably affected by other racial pigmentation factors.

Skin color—in general the brunet factor is dominant over and segregating from fair. Also some genetic factors for skin color are independent of some factors for eye color. There are probably not more than four skin color factors involved, and one of these may be the brunet vs. blonde factor.

Krogman, in a summary of studies of racial crossing,[32] presents a list of physical traits in Man which are considered to be dominant or recessive. He states also,

> There are certain features which appear to act as heritable entities, either with unit characters or with multiple factors. These comprise in general hair-color and eye-color, form of hair, eye-fold, nose, lips, ear, incisor teeth and vertebral border of scapula, head breadth, face length, chin prominence and prognathism, and limb proportions,

31 R. R. Gates, *Human Genetics* (New York, 1946), vol. 2, Chapters 29 and 30.

32 W. M. Krogman, *The Inheritance of Non-pathological Physical Traits in Man* (Eugenical News, vol. 21, no. 6, 1936).

including intra-membral, inter-membral and trunk-limb ratios. These physical characters are non-adaptive, stable and fixed, and may quite reasonably form the basis of the assessment of racial distinctions.[33]

Brues,[34] who has presented a rather complicated work on the inheritance of eye-color, finds that the varieties of human eye-color are the result of various combinations of six factors. The limitations of this paper do not permit a detailed discussion of these factors nor would such a discussion be strictly to the point since Miss Brues concludes that "the limited scope of the present investigation leaves the racial differences out of consideration." However, it is her opinion that because Irish-American and German-American groups have been thus isolated, there is a possibility of indicating racial differences on this basis in the future.

Much is known genetically about many of the physical characteristics of Man, and little is known about others. Of all the characters of Man which have a genetic basis, the most well known are the blood groups. Human blood has been classified into four main categories: O, A, B, and AB. They have been proved to be inherited in confirmation of a theory of triple allelomorphs. The three genes for human blood have been termed the I^A, I^B, and i genes. The combinations of these genes are as follows:

Blood Groups	*Gene Combinations*
O	ii
A	$I^A I^A$ or $I^A i$
B	$I^B I^B$ or $I^B i$
AB	$I^A I^B$

In plotting the frequency of these blood groups in various populations[35] it has been found that nearly half of the European and United States Caucasoids are of blood group 0 and that very few are B or AB; the i gene is somewhat more common among the Negroids than the Caucasoids, but the I^A gene is rather low; the I^A and I^B genes among the Mongoloids is relatively high, but the AB blood group genes are quite variable; among the American Indian tribes the I^A and I^B genes are nearly absent except among the Blackfoot where the I^A gene has a higher frequency than any other known human population.

Three other known classes of human blood, the MM, MN, and NN are

33 *Idem*, p. 144.

34 A. Brues, *A Genetic Analysis of Human Eye Color* (American Journal of Physical Anthropology, n. s., vol. 4, pp. 1-36, 1946).

35 Strandskov, *The Distribution of Human Genes*, table 3, p. 208.

completely independent of the four groups just mentioned. The M-N groups are also inherited, but their occurrence depends upon the presence or absence of one or the other A^M and A^N pairs of genes. The combinations of these genes are as follows:

Blood Group	*Gene Combination*
MM	A^MA^M
MN	A^MA^N
NN	A^NA^N

Neither gene is dominant over the other. The frequency of the gene combinations has also been plotted for various populations,[36] and it is found that the frequencies are about the same for all populations, Caucasoid, Negroid, and Mongoloid.

Recently, Boyd[37] has done considerable work with the Rh blood factors as racial criteria. This series being very complex, will not be considered here in detail. As was noted earlier in this writing there is a high incidence of Rh^0 in Negroid groups; the Rh negative gene is almost absent in the Mongoloid and Pacific groups; and the Rh^s gene is higher in the Australians and Mongoloids than in any other groups.

It is apparent from the foregoing statements that the geneticists have collected a store of information with regard to human blood groups and their frequencies among the various races. In addition, Strandskov[38] finds that a condition known as sickle-shaped erthrocytes, in which the red blood cells assume a characteristic crescent shape, is found to occur only among Negroes. This condition is inherited as an autosomal dominant.

To what does all the data presented by the geneticists add up? It is apparent at the outset that the geneticist does not have his own classification of Man since he utilizes the traditional racial classification in choosing his samples for testing. The purely genotypic classification is shot through with difficulties from the mere fact that little is really known of the gene make-up of humans and that, because of the difficulties involved in acquiring more data, it may be years before more is known. The aforementioned difficulties are adequately summed up by H. S. Jennings,[39] who lists five unfavorable conditions:

36 *Idem*, table 4, p. 209.

37 Boyd, *The Use of Genetically Determined Factors.*

38 Strandskov, *The Distribution of Human Genes*, p. 210.

39 H. S. Jennings, *The Laws of Heredity and Our Present Knowledge of Human Genetics on the Material Side* (in *Scientific Aspects of the Racial Problem*, New York, p. 57 *et. seq.*, 1941).

1. Systematic experimentation in breeding is impossible.
2. The single generation in man is too long.
3. The human family is too small. Larger families are needed for study, such as the 200 or more offspring of the fruit fly.
4. The number of chromosomes in man is large. The fruit fly has 4 pairs, man 24.
5. A large proportion of the characteristics presented for study are disorders, defects, and abnormalities.

These may arise in different cases from defects in different chromosomes, yielding different types of inheritance. Any attempt, therefore, to assign the members of the human population to positions in subdivisions or races on the basis of genotypes must, for the present at least, meet with failure. As has been mentioned, many geneticists hold with the present racial classification as far as *groups* are concerned. Their major criticism lies in the fact that certain overlappings of physical traits make it impossible to classify *individuals* in those racial types.

It is true that the physical anthropologist has set up ideal types and then classified populations as belonging in these various types. Because he has used a large complex of physical characteristics, based on measurable and observable data, he has had to make concessions with regard to the occurrence of certain of these traits within the complex designated for each race. That is, since individuals rarely possess all of the traits listed as common to a certain race it is often necessary to classify people on the basis of the majority of physical traits they hold for that complex. All blue-eyed, blonde, longheaded individuals may not be tall; some, perhaps, are of medium stature or even short. Are those individuals, then, to be eliminated from a certain racial category merely because they do not exhibit *all* of the physical traits held for the ideal type?

Let us suppose that the geneticist sets up a racial classification on a genotypic basis, one which has nothing to do with the present classification. If he uses blood types as a constant he will find that within his type 0, for example, there are some individuals who are colorblind, and others who are not, some who are tasters of phenyl-thio-carbamide and some who are not—for so far as is known there are "tasters" and "non-tasters" in every population, although the frequency of the genes differs from one population to another. The same is true for colorblindness. The geneticist must also make some concessions with regard to his "racial" classification.

This is not to say that the study of human genetics has not presented valuable data on humans in an historical sense. Some information concerning the early

movements of peoples can and has been derived from the study of blood groups.

An indication that the distance between the approach of the geneticist and that of the physical anthropologist to the problem of racial classification may be bridged comes from a paper read before the Sixteenth Annual Meeting of the American Association of Physical Anthropologists, December, 1946.[40] It was stated there that the P-gene has been utilized to some extent and found to be advantageous. A "P-gene" is defined as an

> hereditary phenotypic trait whose distribution in a population can be regularized so that a genetically meaningful comparison between groups can be made quantitatively, even though the genetic relationships of the trait have not yet been analyzed.

Additional work is necessary, however, before adequate conclusions can be drawn with regard to the value of the P-gene in the classification of races.

Perhaps one part of the difficulty which centers in the attempts of both the physical anthropologist and the geneticist to classify mankind into races involves purpose. What is the purpose of racial classification? We can hardly put it down to scientific curiosity entirely, although there may be some individuals who attempt to set up a classification of Homo sapiens into races merely to see whether or not it can be done. Such workers are in a minority, however.

It seems obvious that nothing concrete can be done on any subject having to do with the specific problems concerning Man without first establishing a basis from which to work. That all human beings belong to a single species (H. sapiens) is held as a fact by both physical anthropologist and geneticist alike. Does this, then, not constitute a basis from which to derive facts about mankind? For the working out of broad problems of mankind in general it does. Much can be and has been done along the lines of the relation of man to the other animals. But if we desire to know more about Man and the variations which occur among various groups of people, a more specific breakdown is necessary. The problem, debated at present, of whether or not there are differences in intelligence among people exhibiting varied physical characteristics cannot be investigated without first arranging human beings into some classification according to type. Whether this is done typologically or statistically does not seem to matter so long as the most useable classification results. Problems dealing with human races with regard to climate, general environment, intelligence, susceptibility to diseases, growth, etc., cannot be worked out until a workable classification has been set up. All scientific classifications must be considered tentative, as they may be modified at

40 J. B. Birdsell, *Genes, P-genes and Racial Analysis* (American Journal of Physical Anthropology, n. s., vol. 5, p. 237 [abstract], 1947).

any time by the acquisition of new data which may render the old scheme outmoded.

The geneticist and the physical anthropologist appear to be working toward the same end, that of the classification of Man into races. The approach to the problem is different and it may be that different purposes or needs for such a classification cause the disagreement.

The geneticist is able at present to divide Man into populations on the basis of several inherited characteristics. He has been most successful in the use of blood groups. For his immediate purpose perhaps a "racial" classification of mankind made up of four categories (A, B, AB, and 0) is sufficient. However, to say that a person or group of persons belong to "race" 0 does not tell much about the appearance of that person or group. The physical anthropologist would like to know the color of the skin, eyes, and hair, the shape of the head, face, and nose, stature and many other things about the individual in "race" 0. He could not, obviously, obtain such information from a classification based upon blood groups alone.

The abstract classification of the races of man which has been traditionally used by the physical anthropologist is deplored by the geneticists as being inadequate and even unscientific since it is declared to be based upon the measurement and observation of physical characters which are adaptive and have also selective advantages or disadvantages in an evolutionary sense. If the avowed purpose of physical anthropology, they say, is to trace the evolutionary history of mankind back to its earliest beginnings, then the best method is by consideration of blood groups. For blood groups follow the Mendelian laws of heredity and are non-adaptive or at best adaptive to a slight degree. But, although the physical anthropologist is extremely interested in the evolutionary history of mankind, this is not his sole purpose of study. He is interested in a more complete description of the variations which are exhibited by the members of the species H. sapiens and therefore has based his classification of races on a complex of characters which are passed on from parents to offspring by hereditary processes. These complexes of characters are phenotypic and consequently can be used without elaborate laboratory technique. The physical anthropologist does not deny the value of the genetic approach and indeed is anxious to utilize the advancements in human genetics wherever he can to further his purposes, purposes which may not entirely correspond to those of the geneticist.

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PEYOTISM AMONG THE KIOWA-APACHE AND NEIGHBORING TRIBES

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DURING THE SUMMER of 1948, and again in the late winter and spring of 1949, I had occasion to obtain data, through informants and by observation, concerning the peyote cult among the Kiowa-Apache Indians and neighboring peoples who live in southwestern Oklahoma. Although the major problems with which my research was concerned were much broader in scope, and while the peyote cult was not singled out as a specific subject for intensive study, these data may have the virtue of throwing some light upon the nature and function of Kiowa-Apache peyotism today.[1] Also, because of possible renewed legal threats to the existence of the peyote religion, of which the Kiowa-Apache and their neighbors are aware, certain interesting developments are occurring in what might be termed the "sociology of peyotism."

In the area in which the research took place (in administrative terms the Kiowa, Apache, and Comanche Agency) peyotism is today an extremely live and vital force in the affairs of the Indians. Peyote meetings, usually held on Saturday nights and on holiday occasions, are numerous and inter-tribal in attendance. The meetings I attended were sponsored by Kiowa-Apache hosts, but they were attended also by their co-religionists from such groups as the Kiowa, Comanche, Delaware, and Cheyenne. Informants stated that inter-tribal participation is the rule rather than the exception. In this connection the view is frequently expressed that peyote's power has brought all the tribes into friendly association and has mitigated old hostilities and hatreds.

The peyote cult in this region, among such groups as the Kiowa-Apache and their neighbors, is relatively old. La Barre credits the Kiowa with having first used peyote about 1880.[2] Presumably the Kiowa-Apache began the use of peyote about the same time as the Kiowa, since the two groups have been rather closely associated. The Kiowa-Apache say that an individual called Nayokogał brought the knowledge of peyote to them. An informant stated:

Nayokogał was either a Mescalero or a Lipan. He said his people did not know

1 For the field research opportunity I am indebted to the Viking Fund for its grant of a pre-doctoral fellowship. Thanks are due Professor Morris E. Opler and Dr. John Adair, both of whom read the present manuscript and made valuable suggestions for its improvement.

2 Weston La Barre, *The Peyote Cult* (Yale University Publications in Anthropology, no. 19, 1938), p. 111.

how to use peyote right. He said it was a church. He showed his people how to use it right. Then he came up here and told us how to use it. I guess this was about 1875. He said peyote was a worship.

But the origin of the peyote cult is the subject of a tale presumed to extend farther back historically. I present the story in full as related by an elderly Kiowa-Apache man and translated for me by an interpreter:

There is a story from long ago about the origin of peyote. It goes back to the times the Indians were fighting each other. On the other side of New Mexico a group of Indians was camped, and they were attacked by other bands. The mountains there were very high. The tribe that was attacked got scattered. There was just one woman and her boy left. They were Lipan Apache. It was very hot and dry there. All the water had dried up. They had no food or water and there was none around them.

The woman told her boy, "I am tired and hungry and thirsty. We will rest here. Maybe I will die here. You go on. If you can't find anything maybe you will die somewhere."

It was early in the morning. The boy went out in the mountains. His mother had told him to look around to see if he could see anyone. He walked around. Then something above spoke to him. It said, "I know you are hungry. Look down ahead of you. You will see something green. Eat it." He saw a green plant and dug it up and began to eat it. He looked around and saw many more. He ate some. Soon his hunger was gone, as if he had eaten a lot of meat. He dug some more of the plants up and took them to his mother. He told her of the voice that had spoken to him. She ate some of the plants, and felt as if she had eaten a big meal of meat. Her hunger was gone.

In the middle of the afternoon it was very hot. She said, "I do not know who gave us this, but I am going to pray to him." She prayed for water and to find their people again. Later on a cloud began to darken the sky and there was thunder. Rain fell and water was running through the mountains. They drank and rested there that night.

During the night the woman dreamed. Someone came to her and said, "Look over there and you will see a certain mountain." She looked and saw people moving along the hills. There was a creek nearby. It was east of where she and her boy were lying. In the dream she was told to go up on a high mountain in the morning and look out, and she would see her own people. She was told to take peyote to her people and a way would be made for it.

In the morning they washed and ate some peyote. She told her son of her dream. They went to the mountain and looked out, as she had been told in her dream. She saw people settling down and camping. She knew from her dream that they would be Indians. She and her boy started toward them. One man met them. He recognized them as the lost ones. They were glad to see each other. When they reached the camp he told all the people about them.

The boy had peyote with him. After they got settled the boy asked his mother

to fix him a tipi, off by himself. He said he would go in that night and eat the peyote, and after that he would go into the mountains and lie down. He sat down inside the tipi and put the peyote on the ground, just as it looked when he first saw it. He prayed to the spirit that had shown him the peyote, "You have helped me. When I eat peyote tonight I want you to help me to find a way for it." He had a bow and arrow, and he drummed on the bow-string with the arrow. He sang two songs. He smoked a pipe made of bone from a deer's leg. He drummed and sang all night long. Early in the morning he went to the mountains and stayed there all day and all night. The following morning he came back.

He did this several times. He put up a tipi and sang and drummed all night, and then went to the mountains.

Soon the men began to talk among themselves. They said, "That boy knows something." One time one old man went over to the boy's tipi. He called him and asked, "Are you afraid to let me come in? I want to come in." The boy told him to enter, and the old man sat down beside him. The boy gave him the pipe and he lit it from the fire. Then the boy gave him peyote to eat. Early in the morning they went to the mountains and returned to camp just before dark. The boy's mother had taken the tipi down in the meantime.

After that, people asked the old man what had happened. They said they were all going into the tipi next time.

Not long afterward the boy put up the tipi again. The old man came. Another man came by and asked if he could enter. He was told to enter clockwise and to attend to the fire. The next morning the three of them went to the mountains and stayed until sundown. The boy's mother took the tipi down.

The next day the boy told them he was running short of peyote and would go for more. He went where it was growing and brought back more of it.

The next time they put up a tipi and had a meeting, a fourth man came. They told him to sit on the north side. The next day they all went to the mountains as before. That man asked if others could come, and he was told they were welcome.

When there was a meeting again, all those men came and seated themselves, beginning on the south side and on around the tipi. As meeting after meeting took place, more men came, until the tipi was full.

All the time the boy wondered about improving the sound of his bow and arrow. He cut a long stick and thought, "I'll use this to pray with, so that when I grow old I can use it to walk with." He took a horn and put stones in it to make a rattle. At the next meeting he used those things. As he held the staff and shook the horn, he sang some songs he had composed.

Then he found a container made of wood, already formed. He put water in it and tied a hide over it. He made a drumstick from a yucca stalk. At the next meeting he gave the drum to the man next to him, to drum for him while he sang. In the morning he told the man next to him to untie the drum. He said, "Maybe because of this horn we will get meat. The sound of this drum reminds me of the thunder I heard, and the water in it reminds me of the rain that came. Think of things that will be good to have here." The others learned songs of their own, and soon everything began to fit right in the meetings. Different persons added new things.

The rocks on the drum were to represent tipi poles; the rope represented the rope that holds the tipi poles at the top.

Later, Nayokogał learned of peyote and brought it to us. In time it went north into the Dakotas.

To this day it is our religion. Even today different men add things to make it better. The boy told them long ago to think of things to add to make it better. Nowadays meetings are held on holidays like Thanksgiving and Easter, and the feast has been added to it.[3]

To provide the reader with a picture of the actual performance of the peyote rite and its psychological atmosphere as practised today among the Kiowa-Apache, I shall present a description of a meeting observed in early March, 1949, near Apache, Oklahoma. On the basis of my observations at a subsequent meeting and many discussions with informants, I believe the following is fairly typical.

The tipi was made of Sherman duck canvas, several strips being sewn together. It was placed with the door facing eastward. On the inside, buffalo grass was placed around the outer edges and covered with canvas for the participants to sit on. In the center there was placed the peyote altar, a representation of a quarter moon, made of earth, about six inches high and tapering off at the sides. These physical arrangements were made during the two days preceding the evening of the meeting.

Prior to the beginning of the meeting the drum was assembled and tested in the house of the peyote chief's stepfather. A heavy iron kettle, about ten to twelve inches in diameter was used. The kettle was filled about half full of water, small bits of charcoal were added, and a small piece of buckskin, which had been soaked in water, was stretched over the top of it. Along the sides of the kettle, under the overlapping buckskin, marbles were placed, and the buckskin was tied tightly in place by twisting a rope over and under each of the marbles. The drum was then shaken up and down to wet the buckskin thoroughly, and this was repeated at intervals throughout the entire meeting. The buckskin must be kept wet to maintain a good tone, I was told.

About 8:25 PM the participants entered the tipi to begin the meeting. The peyote chief (a young man of about thirty-five) entered first, carrying a handbag containing the paraphernalia to be used. He was followed by the drum chief and then by various participants. Entry was made clockwise. The chief and drum

3 A comparable tale among the Mescalero Apache has been recorded by Opler (see Morris E. Opler, *A Mescalero Apache Account of the Origin of the Peyote Ceremony*, El Palacio, vol. 52, no. 10, pp. 210-212, 1945). The theme of a person discovering peyote and its powers in a situation of stress is common to both Kiowa-Apache and Mescalero versions, though the details differ.

chief seated themselves on the west side of the tipi, directly opposite the door. The fire chief seated himself just to the left of the door after entering. Prior to this, he had kindled the fire inside the bend of the quarter moon, placing the wood in a V-shape, with the open end toward the door.

As soon as everyone was seated and quiet, the chief said an opening prayer, speaking Kiowa-Apache. The drum chief placed the "Father Peyote," a large button, on the altar directly in front of him, on a piece of sage. As he did so he said a prayer. For the benefit of the non-Apache participants—several Comanche, a Cheyenne, and the observer—the chief spoke in English. He said that the peyote rite was the Indian way of worshipping, and that although he was just a poor man without education, he intended to do his best to conduct the meeting properly. He said that the peyote meeting is open to all and that he welcomed the white visitor. Then he translated his opening prayer: "My father practised this way before me, and before his time, the old men followed it. It is a good road. He has given it to me, and he told me to preserve it and pass it on to the others. It is our way of praying to the man up there who is the master of all of us. Through this herb our prayers reach him, to heal the sick people and look out for our relatives who are away and for the boys in the service still overseas. This meeting tonight is for B. B., my stepfather, and also for my sister, both of whom have been sick recently."

Then the chief opened his handbag and removed the ritual objects. He passed a cloth bag containing corn shucks and Bull Durham tobacco counterclockwise. When it reached the fire chief he rolled a cigarette and passed the bag clockwise, each person rolling a cigarette as the materials reached him. Then a glowing stick from the fire was passed around with which to light the cigarettes. All smoking must be of these materials and so lighted. Smoking is supposed to be an accompaniment to prayer. After each one had finished smoking his cigarette, he placed the butt near the end of the altar closest to him.

At this point the chief passed a small bag containing sage counterclockwise to the fire chief, who took a small amount of it, rolled it between his palms, smelled it, and rubbed himself with it. The sage was passed around clockwise, each person doing the same.

A prayer was uttered by the chief, after which he passed a small paper sack containing peyote buttons, both green and dried, counterclockwise. When it reached the fire chief, it was passed around clockwise, each person taking two buttons and eating them.

After this the singing and drumming began. The drum chief began to drum and the chief sang. As he sang, he shook a gourd rattle in his right hand and

held his staff and a large feather in his left hand. After singing four songs, the chief passed the gourd and staff clockwise to the next man, and the drum was passed by the drum chief clockwise to the chief. After this pair finished four songs, the drum, gourd, and staff again moved clockwise in a similar manner, and so on around the entire circle. A few individuals had feathers of their own which they held as they sang, but most of them held a piece of sage at a slight angle to the staff.

This continued until nearly midnight. The drum and gourd stopped going around when they reached the door and were returned to the chief. The fire chief entered with a bucket of water and placed it just inside the door. At this point the water song was sung by the chief. A cigarette was rolled and lighted by the drum chief and given to the fire chief, who said a prayer. At the conclusion of his prayer, the cigarette was returned to the drum chief, who smoked the remainder of it as he prayed. After spilling a small amount of water on the ground and taking a drink, the fire chief passed the water bucket around, clockwise, each person drinking from it. When it returned to the fire chief, he placed it on the ground, circled the inside of the tipi clockwise, picked up the bucket and took it outside.

At this point, B. B. gave a speech. The substance of it, as translated for me, was that he expressed thanks to those who gave the meeting for him. He said that he had followed the peyote religion for sixty years and that was the reason he had lived to be eighty-four years old. He voiced the hope that he would live to be one hundred years old and that all those present would have long lives. He said that he welcomed the white visitor, and that long ago he had had a vision of the white man coming to learn the Indian peyote religion.

The drumming and singing took up where it had left off and continued until nearly 4:00 AM. During this period, individual participants frequently requested peyote by motioning to the chief, who then passed the bag of peyote buttons in their direction. Several individuals had their eyes closed and made a compulsive drumming motion as the singing and drumming went on. At various times persons would spontaneously mumble prayers. There was one woman present, accompanied by a small child who slept beside her. At about 2:00 AM, during a brief silence as the drum was being passed, she asked the chief if she might have permission to speak. In an hysterical, weeping voice, with tears streaming down her face, she said that she had come to the peyote meeting because her sister and her sister's children, as well as she herself, were ill, and that she was praying for their recovery and her own.

Just before 4:00 AM the chief picked up a small wooden whistle, which had

been placed in a leaning position against the altar at the beginning, and blew it four times. I was told later that this whistling is supposed to wake up "all the living things of the world that are sleeping."

The drumming and singing continued. About 7:00 AM it was daylight. The woman participant was sent out to obtain water. When she entered she placed the water bucket on the ground before her and kneeled, facing the fire. The drum chief prayed. Then he handed her a cigarette that he had rolled and lighted. As the woman smoked the cigarette and prayed, she wept profusely. The cigarette was returned to the drum chief, who smoked the rest of it as he prayed. The woman spilled a little water on the ground and took a drink from the bucket. The water was passed and removed by her in the same manner as at midnight. She was then sent out to get the "breakfast." She entered the tipi with four buckets: one of water, one of corn, one of pemmican, and one of corn gruel. A song was sung by the chief just prior to her entry and three additional songs sung by him immediately following.

The drum was then taken apart by the drum chief and the drumstick placed inside the kettle. The drum was passed around clockwise, each person taking the drumstick and putting the wet end of it in his mouth four times. Some individuals simply rubbed the wet end on their hands and rubbed their heads with their wet hands. The marbles to which the buckskin was tied were passed, each person tapping his body in various places with his fists full of the marbles. The gourd and staff likewise were passed, each one shaking the gourd and uttering a wish for long life and good health, and holding the staff extended along his legs and arms, to make him strong and sound of limb. When the drum returned to the drum chief he slowly poured the water in it over the altar, from end to end.

The containers of food were passed around, each participant eating a spoonful or two from each bucket. When this was completed, the participants began leaving the tipi clockwise, following the man nearest the door. The meeting was over.

In eating peyote, the ritual manner is to hold the peyote button in one hand, usually the right, and motion four times toward the fire, putting the peyote button into the mouth on the fourth motion, and then rubbing the hands over the head, legs, arms, and especially any places where there is sickness or pain. Several people also spit on their hands after chewing the peyote and rubbed the saliva over themselves. Sometimes, upon returning to the tipi after temporarily absenting himself, a person would stand momentarily before the fire and make motions of scooping up the smoke, four times, and inhale deeply from his hand. Periodically throughout the meeting, the chief or drum chief would drop ground cedar needles into the fire.

During the course of the night, participants were free to leave and re-enter the tipi, except at times when the fire chief was fixing the fire, it not being permissible to pass behind him, and during prayers.

The period between the end of the meeting and noon was spent in and around the tipi, smoking, gossiping, and telling stories.

At noon a dinner was served inside B. B.'s house, consisting of boiled beef, canned fruits and vegetables, fried bread, and coffee.

The entire emphasis of the meeting—the reason for calling it, the prayers and speeches—related to sickness and dangers. The behavior and expressed ideology indicated a strong belief in the curative and protective powers of peyote.

The peyotists believe that there is no conflict between their religion and the Bible. An informant stated:

> They tell about peyote in the 14th chapter of Romans in the Bible.[4]
>
> There is a supreme being. God put peyote on earth for the illiterate Indian so he could get the religion of the Bible. God comes to the Indian through peyote.
>
> Peyote is to cure and to bring peace, in the family, among neighbors, and all of mankind.
>
> The peyote religion says there is no end to life. The body decays but the soul has everlasting life. Peyote believers must live cleanly and love their neighbors. They must help the poor and the needy. They must pray and think of God daily.
>
> Peyote believers must not touch liquor or even look at another man's wife.

There is an implicit, deep-rooted faith in the curative powers of peyote. The same informant stated, "Peyote saved me when I had ulcers." Another Kiowa-Apache, who has practised the peyote religion for more than half a century, claimed he was cured during the World War I influenza epidemic by drinking "peyote tea." Following the peyote meeting described above, a Cheyenne participant told me that he had been given up by white doctors thirty years earlier when he had tuberculosis. When he was offered peyote and told that the peyote religion could cure him, he was extremely skeptical, and only with reluctance did he try it. To his surprise it resulted in his cure after a while, and today, he testified, his faith in the peyote religion is unshakeable.

Many peoytists are very explicit in making the frequently heard statement that it is not peyote itself that is the object of worship in their religion. Peyote is conceived of as a medium through which the believer can reach a state of communion with God. Strong objection is voiced to the idea that peyote is an intoxicant or that its effects are in any way like those of alcohol. Peyote has "power"; it "works inside of you"; it gives you a "good feeling, a religious feel-

4 The reference apparently is to Romans, 14:2, which speaks of the eating of herbs.

ing." Quite often the peyote enthusiast will answer inquiries regarding the feeling induced by eating peyote with the advice that the questioner can learn the answer only by trying peyote himself.

Among the Kiowa-Apache today shamanism is no longer practised. In many respects the peyote rite appears to be the contemporary native method of curing. Among the Kiowa, so far as my knowledge goes, there are today but two practising shamans. Interestingly enough, these men are ardent peyotists, and, I am informed, often carry out their curing ceremonies during the course of, or immediately following, peyote meetings.

Having described the peyote rite as practised today and the associated ideology, some remarks may be in order concerning the sociological aspects of it.

In 1918, in response to suggestions made by James Mooney, an early student of the peyote cult, the Indians of this area formed the "Native American Church," together with other Indian groups in Oklahoma, and obtained a charter under Oklahoma state laws.[5] Though the use of peyote and the ritual surrounding it have been under periodic attack from missionary quarters since that time, the legal status of the organization and its activities has been unshaken, and the devotees have felt relatively secure.

Today, however, the picture seems to be changing. While the Methodist churches in the region have been tolerant, or at least indifferent, toward the peyote religion, with the result that many Indians participate in both religions, the attitude of the Baptist church is quite different. The peyote rite is looked upon by the Baptists as idolatrous, and the effects of peyote are alleged by them to be distinctly deleterious, physically, mentally, and morally. During the second period of my fieldwork, for example, a group of students from the Oklahoma Baptist University participated in a radio broadcast over the University of Oklahoma station in which, among other "evils" and "vices" constituting "obstacles to the Indian's progress" they listed peyote. This evoked a great deal of ill feeling among peyotists, and two weeks later, the peyote followers obtained radio time to answer the allegations of the Baptist group. Among the Kiowa-Apache, the peyote believers regard the members of the tribe who attend the Baptist church as opponents of the peyote religion. One informant characterized them as "too much Christian people." Apart from such mutual recriminations, however, the Baptists and peyotists go their separate ways, and I found no evidence of any overt actions on the part of either group to interfere with the practice of the religion of the other.

5 La Barre, *op. cit.*, pp. 170-171.

More serious from the standpoint of Indian feeling and action is the belief that a recent investigation of peyote has been made by the American Medical Association and the fear that legislation may be introduced to classify peyote as a habit-forming drug and bring it under control of narcotics laws. Some of the Kiowa-Apache and associated groups speak, in specific terms, of such an investigation and the threat it may pose; most individuals, however, feel simply that some kind of trouble is in the offing.[6] All of the peyotists feel acutely a need to act in order to fortify the position of the Native American Church against impending threats. But the question of what to do and how to proceed has stimulated the growth of factional differences and individual striving for positions of power and prestige.

An opportunity to gain some insight into the current situation in regard to this was afforded by attendance at a meeting of one group of peyotists, mostly Kiowa, in which the matter was fully aired. The major question was whether an effort should be made to reinstate the state charter of the Native American Church of Oklahoma and thus protect the rights of the peyote religionists of the state, or whether a joint effort should be made with Indians living in other states to obtain a national charter in Washington, D. C. The feeling of the entire group at this particular meeting favored the first alternative. The grounds for the preference are interesting: Indian groups in other states, it was said, are guilty in some instances of the misuse of peyote in two respects—some use liquor while still under the influence of peyote; some commercialize the use of peyote by charging a small amount of money for each peyote button consumed at meetings. Should the Indians of Oklahoma become identified with such misuse of peyote, the argument goes, as they would under a national charter, it would make the peyote religion everywhere vulnerable to attack. So runs the viewpoint in favor of a state charter only. There was some comment at the meeting to the effect that when an understanding could be reached with the groups who allegedly misuse peyote, and the ritual unified and standardized, then and only then would it be

6 The belief on the part of some of the Indians that the American Medical Association has made an investigation of peyote and taken a stand on its usage is apparently incorrect, for in a personal communication to the present writer, Austin Smith, M.D., Secretary to the Council on Pharmacy and Chemistry of the American Medical Association states: "We can find no record of any study by the American Medical Association of the uses and properties of peyote." The Indians' notion that the A.M.A. has made such an investigation may be related to the fact that an article by Drs W. F. Braasch, B. J. Branton, and A. J. Chesley entitled *Survey of Medical Care Among the Upper Midwest Indians* appeared in the Journal of the American Medical Association for January 22, 1949, in which the use of peyote by Indians is discussed from these writers' viewpoint.

wise to seek a charter from the federal government for a Native American Church of the United States.[7]

The leaders of the opposition faction, seeking a national charter, seem to be two prominent individuals, one Cheyenne, the other Kiowa-Apache. The latter, a self-styled chief when outside his community, was, at the time I left the field, considering an automobile trip to Washington to prevail upon the federal government for a national peyote charter. This group appears to constitute a small minority among peyotists in the area, and to its leaders are ascribed the not-unlikely motives of seeking high offices in a nationally organized Native American Church.

The intensity of the feelings and actions of peyote adherents and their social division in the face of potential threats from outside sources is understandable in the context of the socio-cultural conditions prevailing in the area and in terms of the past. The peyote religion functions as an emotional outlet and partial integrating force in a situation which is generally characterized by extreme individualism, economic insecurity, and marked health anxieties. The absence of anything resembling an integrated tribal or inter-tribal community; the individualism, jealousies, and resentments engendered by the system of individual land allotments; the feeling of dependency fostered by paternalistic government control over social and economic matters—all these provide fertile ground for the growth of mutual suspicion and distrust between individuals and between groups. It is in this context that the peyote cult constitutes the religious symbol of what might be called a "Pan Indian" movement, asserting minority group solidarity within the larger cultural framework.

It is safe to predict that the peyote cult will continue, under these circumstances, to constitute a major form of emotional outlet and that these people will struggle with great tenacity, if not unanimity, against any efforts of outside forces to end it.

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7 The considerable concern about, and implied disapproval of ritualistic differences, suggests the question whether we might not have here the inception of a process akin to denominational splitting.